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ABSTRACT

This conference brought together directors of research grants and research-based contracts funded by the Division of Innovation and Development of the Office of Special Education and Rehabilitation Services (OSERS). The proceedings contain copies of three speeches given at the conference, along with transcripts of the question-and-answer sessions that followed. "The Social Context of Research Design: From Theory to Practice in the Care and Education of Retarded Individuals" by Edward Zigler offers a historical perspective on the issues of deinstitutionalization and mainstreaming, focusing on changes in views and practices toward retarded individuals over time and the role of science in the mental retardation field. "Naturalistic Inquiry: Politics and Implications for Special Education" by Yvonna Lincoln offers a naturalistic model for understanding reality, subject-object dualism, generalization, causality, the role of values, and the treatment of rigor, and outlines the implications of the new paradigm for special education researchers. "View from OSERS" by Martin Kaufman discusses the review system for research proposals at the Office of Special Education and Rehabilitative Services, the selection of reviewers, OSERS' difficulty in communicating its intentions, and current research priorities. In addition to the speeches, two papers from a panel discussion "What is Effectiveness?" moderated by Naomi Zigmond are included: "Some Thoughts on Effective Intervention for Handicapped Preschoolers" (Phillip Strain) and "Makers of Effectiveness at the Secondary Level in Special Education" (Eugene Edgar). (JDD)

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INTRODUCTION

On July 10-12, 1988, the Office of Special Education Programs of the U.S. Department of Education held its third annual Research Project Directors' Conference. Each year the directors of research grants and research-based contracts funded by the Division of Innovation and Development are invited to attend this meeting. OSEP instituted this annual researchers' conference in order to strengthen communication within the research community and to provide individual researchers with an opportunity to view the research process from a wider, more integrated perspective.

The conference is planned by a committee of senior researchers nominated at the close of each year's meeting. The 1988 meeting was planned by Naomi Zigmond, University of Pittsburgh (Committee Chair), Jim Ysseldyke, University of Minnesota, Luanna Meyer, Syracuse University, Ann Kaiser, Vanderbilt University, and Charles Greenwood, University of Kansas.

The 1988 meeting featured a combination of general sessions, small-group discussions on a variety of research issues and content areas, and a panel discussion on determining and measuring effectiveness in special education. This proceedings includes copies of the speeches given by Edward F. Zigler, Yvonna Lincoln and Martin J. Kaufman, with transcripts of the question-and-answer sessions that followed each, and two of the three panel-discussion presentations.

Planning for the 1989 conference has already begun, based on the evaluations of the 1988 conference. We look forward to another successful year.

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The Social Context of Research Design:
From Theory to Practice in the Care and Education Of Retarded Individuals

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From Theory to Practice in the Care and Education of Retarded Individuals

A pressing problem in the field of mental retardation is how to constructively intervene with and care for retarded individuals. There may be no issue or set of issues that is so beset with stridency, with polarized views, and with discoveries of "new" solutions. Though the current issues are complex, they are not new; nor are many of their purported solutions. Issues and solutions emphasized at any particular time often represent the swinging of the historical pendulum, whose path and speed are directed not only by the findings of scientific research, but also by political, economic, and social forces that often do not result in the clearest view of the problem.

Today I will address the issues of deinstitutionalization and mainstreaming from a perspective based on history. An historical perspective is important because a knowledge of the field's history can help us to prevent an overselling of present "solutions" to age-old problems. Over the years, we have become wary of people who declare that we need not examine alternatives, and distrustful of any select group of experts who claim they already know the single best solution through the exercise of their common sense. Experience demonstrates that common sense often proves to be more common than sensible. To quote George Santayana, "progress, far from consisting in change, depends on retentiveness. . . . Those who cannot remember the past are condemned to relive it." It is my hope that we in the mental retardation field can be guarded and realistic in what we promise; perfection will no doubt always evade us.

Before discussing the history of mainstreaming and deinstitutionalization, I want to alert you to several themes you will hear running throughout this talk. I already alluded to the first, that views and practices toward retarded individuals change over time. Consider for a moment the field's views toward special education. In a major textbook written over 20 years ago, Robinson and Robinson concluded: "the consensus of special educators today definitely favors special class placement for the mildly retarded." Two decades later, virtually all mildly retarded children are in mainstreamed classes. Yesterday's orthodoxy has become today's heretical view.

A second theme involves determining the role of science and scientists in the mental retardation field: Should scientists gather information to help others make informed choices, as opposed to advocating for retarded individuals? Should scientists make choices for retarded persons and their families? Can scientists draw firm conclusions concerning care and intervention, especially given the fact that fashions change? These are only some of the questions, and themes, of the history I will turn to now.

The historical vantage point from 1988 allows us to see in bold relief the errors of the past, and provides a clearer perspective for our own efforts in the future. In 1848, the first private facility designed specifically to care for retarded people opened in Barre, Massachusetts. It was followed two years later by the United States' first public facility in Boston, founded by Samuel Gridley Howe. This is now the Fernald State School in Waltham. By 1890, there were approximately 20 residential schools in 15 states. In the field of special education, New York City and

Cleveland first established school classes for "problem children" in the 1870s. Providence originated the first classes specifically designed for mentally retarded children in 1894.

A host of special services for retarded persons also began during this period. It was a time of general optimism concerning the advancement of the social, political, scientific, and moral qualities of humankind. This spirit favored the development of numerous social institutions and services, including schools for blind, deaf, and mentally ill persons, and the establishment of the professions of medicine, nursing, education, and social work. As Best noted, "Probably the world has never known, before or since, such a pouring out of sympathy for the afflicted of society, a more zealous resolve to speed their relief, nor a more ardent faith in the possibilities of education."

The founders of American "training schools"--as institutions for retarded people were then called--and others concerned with the education of retarded children were influenced by this 19th century belief in progress. In particular, they were excited by news of the so-called "physiological education" developed by Edouard Seguin in France. Yet as Seguin's ideas reached the United States, his most influential contribution to the training of retarded persons turned out to be his view of what he called "moral education." This notion disavowed inhumane therapies and harsh discipline. Instead, the goal of moral education was a loving relationship between the teacher and pupil and the gentle bending of the will of the retarded student to that of the teacher. Proponents of moral education believed that teaching retarded children involved "reawakening" them into a normal human existence. In essence, Seguin's legacy was the expectation that retarded individuals could be made normal.

Of course this expectation proved too optimistic, and by the late 19th and early 20th centuries views about retarded people had changed completely. At this point the causes of retardation were thought to be primarily genetic. The popularity of newly-developed intelligence tests demonstrated the discouraging finding that even at such outstanding institutions as the Vineland Training School, intelligence levels of retarded residents were failing to improve. These factors led to the popular "legend of the feeble-minded" which, in the harsh words of Walter Fernald, characterized retarded people as:

> "a parasitic, predatory class, never capable of self-support or of managing their own affairs. They cause unutterable sorrow at home and are a menace and danger to the community. Every feebleminded person is a potential criminal, needing only the proper environment and opportunity for the development and expression of his criminal tendencies."

In fairness, it should be noted that this legend of the feebleminded was not universally accepted. Fernald himself expressed great disappointment over the lack of improvement of retarded persons on Goddard's Binet-Simon tests. He later conducted a study of the life status of 646 retarded children released from the Waverly facility. The findings of this study, showing that over half of the former residents had made at least a fair adjustment to life outside the institution, led to a change in Fernald's views. He conceded that "We honestly believed that nearly all of these people should remain in the institution indefinitely, but the survey shows that there are bad defectives and good defectives, and that a few defectives do not need or deserve life-long segregation." Fernald deserves much credit for changing his mind when confronted with new evidence. He makes a fine model for those of us in the field today.

During the earlier period, when pessimism was rife regarding retarded people, advocates of eugenics managed to pass legislation in twenty-five states mandating the sterilization of retarded people. Given these misguided fears and beliefs about retarded people, it becomes more comprehensible, although not excusable, why large institutions were built far from populated areas and filled to capacity between the 1920s and the 1960s.

Turning to the history of schooling for mentally retarded children, we note that special education programs were initiated to remove the most difficult students from the regular class setting. Thus around the turn of the century, special classes included not only mentally retarded children, but delinquent, truant, and emotionally disturbed students as well. When their formal schooling was over, most retarded children were expected to enter institutions. In 1920, Ada Fitts, supervisor of special classes in Boston, stated that sending these children directly from school to the institution would "safeguard the public from inefficiency, unemployment, pauperism, vagrancy, degeneracy, and all the other social consequences of feeble-mindedness." Over the years, this negative stereotype of mentally retarded individuals caused them to be treated as if all attempts to educate them were futile.

The establishment of the Council for Exceptional Children in the early 1920s proved to be a harbinger of changing attitudes and diverse approaches toward retarded persons. Even in the early years of the Council's life, it played an important role in advancing educational opportunities for retarded children. Unfortunately, the very optimism it inspired for treatment of these students also led to the expectation that special education could radically elevate their levels of functioning. In pursuit of this unrealistic hope, experts in special education advocated widely differing approaches to schooling for retarded people. Thus even in the early years of special education, the field was rife with uncertainty and controversy over what direction it should take.

By the 1950s, the public education system had begun to understand that most retarded students could support themselves after their school years. Thus, public schools took increasing responsibility for their trainable retarded students. They did so for several reasons. First, moderately retarded children were living longer. Many parents did not want to institutionalize their children because of the deplorable reputations of large facilities. Second, the newly formed Association for Retarded Children lobbied effectively. They produced position statements such as the Education Bill of Rights for the Retarded Child, adopted in October of 1953. This bill proclaimed the right of every retarded child to a "program of education and training suited to his particular needs," and gained many a sympathetic ear. Finally, the shift of educational policy-making from local school boards to state agencies increased the opportunities for retarded children. As a result of this activity, enrollment in special education classes jumped from 5,000 in 1953 to 30,000 a decade later.

The 1960s and 1970s saw the pendulum swing back again to criticism of special education. Since special education classes were generally smaller and more individualized than regular classes, school systems often balked at their increased costs. In 1962, G. Orville Johnson reexamined a number of post-school follow-up studies and found it "paradoxical that mentally handicapped children, having teachers especially trained, having more money per capita spent on their education, and being enrolled in classes with fewer children and a program designed to provide for their unique needs, should be accomplishing the objectives of their education at the same or at a lower level than similar mentally handicapped children who have not had these advantages and have been forced to remain in the regular grades." In 1964, Samuel Kirk published a research review arguing that despite the rapid increase in special classes for mentally retarded children, "there is only sporadic research evidence which justifies this increase." And, as I will discuss in a moment, in 1968, Lloyd Dunn reviewed the literature and published a paper that seriously questioned both the efficacy of special classes for mildly mentally retarded persons and their widespread installation.

We see, then, parallel, though not identical histories in the residential and educational treatment of retarded persons. Both were outgrowths of 19th century views of progress, and both featured ambivalence about the aims, costs, and provisions of services. In addition, both have shared the overly optimistic and overly pessimistic views about retarded individuals prevalent in society at large.

Just as institutions and special education have overlapping early histories, so too can their more recent histories be seen as similar. In particular, both share in the recent move toward normalization, a view that has its own historical and philosophical underpinnings.

Historically, the widespread deinstitutionalization movement in the United States began with a series of indictments of large institutions during the 1960s that shocked the American public. In 1966, Blatt and Kaplan published a book of photographs entitled Christmas in Purgatory that depicted the deplorable conditions in several large institutions. The photographs showed poorly clothed or naked residents, residents locked in rooms whose only view out was a rectangular piece of glass measuring 3 X 6 inches, and large, lonely dayrooms smeared with excrement on walls, floors, and even ceilings. The now-defunct Look magazine published many of the photographs. The ensuing public response was greater than to any previous piece in the magazine. Equally inhumane conditions at the Willowbrook facility on Staten Island in New York were exposed and widely publicized both by the visit of Senator Robert F. Kennedy and by the television journalist Geraldo Rivera. At the same time, advocacy groups such as the National Association for Retarded Citizens were effective in exerting pressure to change existing institutions.

Similarly in the field of special education, there were historical forces helping to bring about normalized placements of special needs children. The U.S. Supreme Court decided in Brown v.

Board of Education that racially segregated classrooms could not be "separate but equal," and this reasoning permeated the thinking of special educators. A series of court decisions ruled against special class placement for black and minority children. Finally, in an influential article in 1968, Lloyd Dunn argued that special class placements did not more effectively foster development in retarded children, and that these placements stigmatized and segregated special needs students. Dunn felt that contemporary educational technologies put to use in regular, mainstreamed classrooms would better serve retarded children. As in the case of the large institutions, then, a series of historical forces were at work to promote normalized placements.

Philosophically, the normalization movement originated in Scandinavia and spread to the United States. According to Nirje, normalization is based on the idea that each person has the right to experience a style of life that is normal within his or her own culture. Thus, retarded individuals should experience a normal rhythm to the day, such as getting up in the morning, eating meals at certain times, and going to bed at an age-appropriate hour. There should be a normal rhythm to the year, such as enjoying vacations and holidays. The life span should also be normal, such as moving from the world of school to the world of work. Under the original concept of normalization, all individuals should be allowed the right to participate in activities common to same-aged members of the society.

With Wolfensberger's influential book, however, the focus shifted from normalization of lifestyles to a normalization of services. Retarded and otherwise disabled individuals could best be served only when the services themselves were normal. That is, services for retarded individuals should be the same as, and no more restrictive than, services available to the nonretarded population. Indeed, Wolfensberger and others have equated a normalization of services with the attainment of more normal lifestyles. Successful environments for retarded individuals are those that are most "like normal," not necessarily those that most facilitate the development and adaptation of retarded individuals. The recently developed PASS model, which evaluates the adequacy of living alternatives based on the degree to which they approximate normal living settings, is an outgrowth of Wolfensberger's view of normalization.

One significant effect of the theoretical shift from normalization of lifestyles to normalized service delivery involves the interpretation of the 1975 Education for All Handicapped Children Act. This law assures a "free appropriate public education" for all retarded children, no matter how impaired. Certainly the law is an important turning point in the education of retarded children. In practice, however, its provision that they be educated in the least restrictive educational setting has usually been translated into a regular class placement for most educable mentally retarded children.

Again, in theory, everyone agrees that retarded children should live as normal a life as possible. Landesman and Butterfield refer to this as the consensus over the goals of normalization. Whether mainstreamed classrooms provide the best educational opportunities for retarded children is another matter. As Susan Muenchow and I argued, the proof of mainstreaming lies in its implementation. And in practice, findings are mixed. Educable mentally retarded children in special education and mainstreamed classes seem to perform equally on academic achievement. Mainstreamed students show higher social skills but are stigmatized by their nonhandicapped peers. Levels of racial segregation, another of Dunn's original reasons for promoting mainstreamed classes, appear about equal in the two settings. Further, the hope that EMR children will learn through imitation the appropriate social behaviors of their nonhandicapped peers seems not to have been realized. Gresham writes that without sufficient training, "there is little empirical evidence to suggest that integration of handicapped subjects into regular classrooms will result in beneficial modeling effects." In addition, the nature of the classes in which the mainstreamed child participates, the methods of teaching, and the type of social interactions that take place have all suffered from a lack of research. Thus, we have not yet come up with an unambiguous answer to the simple question of whether segregated or integrated placement is best.

Similarly in the institutionalization area, almost every study shows that at least some large institutions are less restrictive than are some smaller settings. Even across different large institutions, quality varies enormously. In comparing two large state institutions, Earl Butterfield and I showed that size was unrelated to the atmosphere. In the first institution, every effort was made to provide a homelike environment. Meals were prepared in the living units and eaten in small groups. The frequent social events were co-ed, and the atmosphere encouraged responsibility on the part of the residents. In the second institution, social events were segregated by sex. Meals were prepared by staff, residents ate in a large central dining room, and emphasis was placed upon external control by the staff. Another example of striking differences in large institutions can be found in Burton Blatt's book, The Family Papers. He describes the Seaside Regional Center in Connecticut in these terms: "even though it serves as a residential facility for over a hundred people, it has not fallen into the mood of hopelessness and monotony or produced the attitudes of indifference and degradation that continue at large institutions." Landesman and Butterfield note that these variations across different facilities of the same type are significant, and are sometimes greater than differences observed between different forms of residential care. One must be more concerned with the specific conditions within the institution rather than the size of the institution per se.

There are other examples showing that the equation of normalized services and normalized lifestyles is far from perfect. At the Vineland Training School in New Jersey, for instance, staff have developed a group home that is specifically designed to serve clients with Prader-Willi syndrome. The home is replete with staff supervision and client participation in areas of food choice, preparation, and intake. Behavior modification techniques are used to control behavior problems. This so-called specialized treatment group home has produced reduction in both weight and behavior problems in Prader-Willi clients. At the same time, it is both restrictive and consciously specialized, showing that an improved quality of life is sometimes facilitated through non-normalized delivery of services.

The problem, then, is that we in the field are making social policy and individual case decisions on the assumption that normalization of services equals more normal styles of life for retarded individuals. Indeed, in testimony I gave before Congress in 1976, I described normalization as a banner in search of some data. Adherents of the normalization approach wave this banner more for the emotional catharsis it provides them than for its usefulness in prescribing appropriate living settings or educational opportunities for retarded individuals. If normalization means only that large institutions should be closed and retarded children mainstreamed, then more thought must be given to the normalization issue as it concerns a better way of life for retarded persons.

In attempting to get beyond the label of normalization, we must first acknowledge that too often, workers have been concerned only with the physical settings of services for retarded individuals. We have conceptualized institutions, group homes, special education and mainstreamed classes, only as places, not as places within which interactions occur. To use Bronfenbrenner's terminology, we have fallen victim to a "social address" model of the environment, one in which the only variable of importance is where the services are delivered.

A better strategy involves consideration of the actual social and psychological characteristics within each individual setting. In particular, we need specific knowledge about how each type of setting, and each example within each setting, influences those social psychological variables that impinge upon the person's everyday life. This is true whether a person resides in an institution or, more commonly, in a community residence. For example, in order to optimize the development of retarded persons, we know that caretaker continuity from one or a small number of adults is important; that socially fulfilling interactions with friends and acquaintances must be encouraged; that the opportunity for enjoyable and stimulating activities and an appropriate physical environment must be provided. Yet only when we know the degree to which these basic human needs are provided to retarded people can we begin to make useful interventions, if they do become necessary. In short, we have been arguing the issues of normalization, deinstitutionalization, and mainstreaming at a much too simple level.

My proposal, then, is that the services provided for each retarded individual be matched to that person's needs, regardless of setting. As concerns residences, this proposal would allow a role for the large central institution. In the realm of education, it would allow a place for special education classes and special schools. In short, it is my hope that all settings be improved, that the effects of each be evaluated, and that we continue to work for the best setting for each retarded individual.

This suggestion is not really too lofty or abstract. Already workers are struggling with the appropriate role for each of several service delivery settings for retarded individuals. Led by Marie Crissey, Marvin Kivitz and Marvin Rosen, workers at Elwyn Institutes in Pennsylvania and the Vineland Training School are attempting to determine the appropriate role of the large central institution within a continuum of services. One of their suggestions is that the large institution could oversee a centralized network of services to retarded persons throughout their lifespans. The institution could serve as an information and referral source to parents. It could provide short-term, long-term, and supportive care for retarded people of all ages, and could serve as a liaison to public schools, vocational workshops, hospitals, and other agencies serving retarded populations.

Second, large institutions could train new generations of professionals to work with retarded persons. As centralized facilities which coordinate a network of services, large institutions could give workers the opportunity to gain experience with retarded individuals who vary in age, diagnosis, and level of functioning. This teaching mission is one that institutions such as the Vineland Training School have historically performed.

The third suggestion involves the continued use of large residential facilities as a full-time living place for retarded people. Today residents of large institutions are mainly those who are the most severely retarded, multiply disabled, or who show maladaptive behaviors. Thus, there appears to be a continuing need for these facilities, at least for the present. Still, this possibility can only be entertained if progress occurs in making institutions more humane living settings. I am concerned that the size and isolation--bureaucratic as well as physical--of many of these institutions may make such reforms difficult. Still, the achievements of many workers in the field convinces me that institutions can work. In addition, if it is possible to humanize large institutions, several of their unique advantages could be exploited: the very self-contained nature of these facilities would allow relatively easy scrutiny to make sure that humane standards of care are routinely met. As George Tarjan instructed some years ago, if there are several hundred people in one building or set of buildings, they're visible; abuses may be harder to conceal and people will advocate for the residents within. But when those same residents are split up, with six in one location and eight in another, they vanish from public view.

Another advantage of a large, well-run facility is that parents know the institution will be available for their child as they themselves get older. The child's lifelong security would be assured. In short, institutions would provide the benefits for families and their retarded children that Oliver Sacks describes in relation to hospitals for severely emotionally disturbed individuals:

> "Hospitals, state hospitals, are often seen as 'total institutions' in Erving Goffman's sense, geared mainly to the degradation of patients. Doubtless this happens, and on a vast scale. But they may also be 'asylums' in the best sense of the word, a sense perhaps scarcely allowed by Goffman: places that provide a refuge for the tormented, storm-tossed soul, provide it with just that mixture of order and freedom of which it stands in such need."

Thus, whereas the goal of the institution would remain the most normalized style of life as possible, a short- or long-term refuge for retarded individuals and their families would be maintained.

Likewise with regard to special education services, a range of alternatives must be maintained. In particular, we must remember that the Education for the Handicapped Act specifically declared that all disabled children should be educated with nonhandicapped children to the extent possible. The word "mainstreaming" does not occur in the law itself, but is an outgrowth of the movement to normalize services. Many experts in the field have begun to express reservations about how common mainstreaming has become. The Kennedy Foundation is presently trying to call together a group of experts to take another look at the mainstreaming practice and to determine what sort of education is ideal for mentally retarded children.

Presently, the mainstreaming issue is clouded by the fact that it is based on political and philosophical justifications rather than on any scientific evidence regarding the best school placements for children with particular handicaps. We need more research work on specific problems, rather than attempts at political panaceas. In the words of one parent, these panaceas only guarantee every child an equal shot at a mediocre education. And as Gottlieb noted, an "appropriate education for mentally retarded children has not yet been developed."

Hopefully, this review of history has made explicit the essential tension many of us feel as scientists and practitioners in the mental retardation field. On the one hand, it is our duty to gather and evaluate information, to participate in our work as responsible scientists. Yet all too often, each side of the normalization debate has lapsed into an advocacy or apologist role vis-a-vis group homes or large institutions, or mainstreamed versus special education classrooms. This sort of dogmatism intrudes as well into professional advice concerning the best placements for each individual retarded person. Today families who institutionalize a member are made to feel inadequate or guilty, and these are problems which can be as long-lasting and hurtful as the actual difficulties of dealing with a retarded loved one. Scientists such as ourselves, acting *as scientists*, have a clearly defined role: to obtain information by using our most sophisticated methodology, to give our information to people who need it, and to allow those people to make their own choices.

In a more general sense, we have a special responsibility not only to conduct research, but to advocate for better lives for retarded persons. Obviously, differences of opinion are to be expected, as workers have and will continue to disagree about these issues. Still, given their importance, our mission must be to improve all residential and educational alternatives, to evaluate the effects of each, and to advocate for the mentally retarded population, always with history, science, and humaneness as our guides. If fortunate, we as a discipline will contribute to bettering the lives of retarded persons, while simultaneously lessening our own stridency and polarization concerning the care and education of retarded individuals.

Edward F. Zigler -- FROM THEORY TO PRACTICE IN THE CARE AND
EDUCATION OF RETARDED INDIVIDUALS

QUESTION & ANSWER SESSION

Q. I've always thought of mainstreaming and normalization more as statements of social values rather than statements of theories or theories that would lead to technologies. And I would have an interpretation of much of the same literature that you have gone through as being questionable, mostly from the point of fidelity of implementation. So I don't know that we have "a" definition of mainstreaming. I think mainstreaming is a desired outcome; perhaps that's what makes it a social value. I don't know that we have "a" definition of normalization that we can test in some ways, that people have implemented in some standardized fashion. I don't know what my question is, other than that I have a different view, I think, of normalization and integration, or mainstreaming, than you've presented in your talk today.

A. The fact is, you're right, and I don't think we're disagreeing about it. I believe part of the problem is that we really don't know what we're talking about when we use these terms. They haven't been carefully defined. Normalization can mean very different things to different people. Mainstreaming is not really a set practice but is a continuum. It seems to me these terms have become shibboleths rather than hard formulations. So your very question serves to underline what I consider to be one of the points of the address: although we're not really clear what we're talking about, a lot of people continue to use the words. That's what I meant by saying that these are good banners, that they sound well, but what do they mean? The fact, though, is they have had real impact in the real world. On the mainstreaming front, there's no question that children who used to be in special education are now in mainstreamed classes. By the same token, moving to institutionalization (which I see as parallel), some years ago professionals routinely advised parents to institutionalize retarded children. Today, if a parent tells us, "I have to institutionalize this child--my family's going under," we consider that a kind of a loss for ourselves in some way--that we've failed, or they've failed. So, your question helps to underline what I've been trying to say. Another question.

Q. Ed, a number of people have been commenting on the disappearance of mild mentally retarded children. There seems to be fewer and fewer showing up now. And I just wanted to ask you if you agree that this is a phenomenon that is in fact occurring, and if it is, what are your thoughts on why.

A. I remember a few years ago, Jim, when you and I and some of our colleagues went to Sweden, at the request of the Kennedy Foundation. They were convinced that the rate of retardation in Sweden was 1/15 of what it was in the United States. You had one explanation for these numbers. My own was that if the identification process was done carefully, there were no differences. I think that this notion that mild mentally retarded children are disappearing is--well, nonsense. My full view of this matter is contained in my recent book, Understanding Mental Retardation, written with Bob Hodapp. I continue to believe that the big bulk of the mildly retarded population simply represents the lower portion of the normal distribution of intelligence that's predicted from any polygenic model and from the gene pool. So the notion that we're ever going to see populations in which everyone is above the mean on some measure like an intelligence test has got to be nonsensical.

In the Zigler and Hodapp book we did a careful study of prevalence, and I think one of the more surprising outcomes of that analysis was contrary to the thrust of your question, Jim. I have argued, and now is the time to go on and test it, that in the United States today there are maybe up to a million children who are retarded but currently cannot be labeled as such. The polygenic model says that among parents with normal or even superior intelligence, through the process of polygenic mix, a certain number are going to have retarded children, just as two retarded parents can have a nonretarded child. I won't burden you with the genetic equations for this, but they are fairly simple. That says to me that we have a whole group of mildly retarded children in our society who have middle-class parents. Now take this case--you have a little boy in school who is not doing very well. You give him an I.Q. test and he scores 60.

Well, what kind of retardation is it? You look at the parents. Both parents are college graduates and are in the middle class. It can't be cultural familial retardation--he doesn't meet the criteria of AAMR on that. You look for organic causes and find none. What possible diagnosis could you give that child?

Not long ago when I was kind of at sixes and sevens, I asked my old friend, George Tarjan, "George, according to my analysis, there's got to be nearly a million of these children out there. I've never seen one case history of one such child Do they exist? Have you ever bumped into any?" And George said, "Oh yes, Ed. We call that the Hollywood Syndrome." I said, "Why the Hollywood Syndrome?" He said, "Well, these are children whose parents really don't want the label mentally retarded, and they shop around until they get a label they can accept. And professionals help these parents in two ways." He calls them the physician bashers. These are M.D.s, usually pediatricians, who say offhandedly, "What happened is that when the child was born, there was some minimal brain damage." So the parents can live with that. The second way is to give them a a diagnosis they're comfortable with. Today the diagnosis of choice is "learning disabled." My hunch is that "learning disabled" is really a wastebasket category that is acceptable to parents who are shopping for a label that doesn't sound harsh or permanent. There are a lot of ways of testing this formulation, and I plan to do this over the next couple of years. But if I'm right, there are probably way more mildly retarded children than we've ever recognized. Another question.

Q. I got from your talk the idea that our knowledge base in several critical areas is less well developed than it should be, even though research has been going on for ten, twenty years or longer. Why do you think that the knowledge base is not further along with respect to some critical issues in special education? What kind of obstacles have we faced? Are any of them different from what you would find in medicine or other complex areas? And what can we do institutionally, collectively, to improve that knowledge base in a reasonably quick period of time?

A. That's a good question, and I've given it quite a bit of thought because I feel somewhat guilty, especially when I have to face the young workers among you. Some of us who are Jim's age and mine are fortunate. We lived through what I consider to be the golden age of research in mental retardation. A lot of money, a lot of support, a lot of interest, and some very good theorists came along. Those were the years from about 1955 to 1970, when it ended pretty precipitously. You people here are one of the few remnants, thanks to the wisdom of the Office of Education. I guess I took the wrong side when I was a party to the big debate when we set up NICHD. There was some thought that we should have been an institute of retardation. I was one who argued "No, you'll understand retardation best by understanding normal human growth and development." Being in the winning camp wouldn't have made me so happy if I knew that later on a director of NICHD would essentially cut MR research off at the knees. That's one of the few times I've taken up cudgels in print and argued against the director of NICHD by name.

Another group that I think has abandoned us (by "us" I mean researchers) is the National Association for Retarded Citizens. Many years ago I was the first and I guess only recipient of their award for scholarship in the social and behavioral sciences. They had one in medicine and one in our area. In addition to that, they used to give seed grants to young investigators. I was on the committee that would hand out this money--ten, fifteen thousand. You all know that when you're starting your research, this type of grant is very helpful. What happened has to do with normalization, what we've been talking about today. Once an organization thinks it has the answers and is ready to go on to advocacy, there's no reason to do any more research. If you truly accept a philosophic position, then why study any more. It essentially ends research. I'm afraid that's what happened to NARC. They became advocates for a position on which they felt very strongly. Not only did they give up their own research activities, but they essentially quit going up on the Hill to argue in behalf of research. When scientists like you and me go up and argue for money for ourselves, it appears very self-serving. When parents of retarded children (represented by NARC) do it, it's not self-serving. So, we lost an avid supporter and ally.

I have great respect for what the Kennedy Foundation has done for many years. They used to have a research award and a meeting once a year. It gave the field status and made young people in the field feel that they were a part of something that somebody cared about. Today I see the Kennedy group doubling back to a research orientation. There are a number of these forces--including NARC and NICHD--that are beginning to come back. The Office of Education, starting way back in Jim Gallagher's days and continuing to the present, has stuck it out, so I don't want to paint too bleak a picture.

There's nothing very profound in my answer to you, Craig. You can't do this work with mirrors. It takes money; it takes forces that help us get the money. I don't see that money forthcoming until more of us stand up and say, "Hey, there's still a lot we don't know" and again assert that our practice can outdistance our theory and our science. We have not made that case to decision-makers in NARC. I'm not sure why. I will continue to try to do it, because the field of research historically has its ups and downs, and now is about time for an up. It was a very bleak field for many, many years before the '50s. Thirty years ago, there was a book that perhaps all of us should read because it was a classic in its time. It was called Mental Subnormality, published in 1958 by Masland, Sarason, and Gladwin. In that book, my colleague and very close friend, Seymour Sarason, wrote that he found research in mental retardation to be wanting. He offered the explanation that people who are not too bright themselves are attracted to the study of retardation. Well, that was kind of a challenge for me. But just about the time he said that, we saw this tremendous upswing.

Just let me name some of the names. At that point in time we had only one theory, and it directed practice. We had the old Lewin-Kounin rigidity theory, which stated that by basic nature retarded individuals were rigid, that they loved to do perseverative things. So, what do you teach them to do? You of course teach them to do repetitive tasks over and over and over because it matches their basic nature. This theory explains why we were very reluctant to do any counseling, any therapy with retarded individuals--because the sine qua non of therapy is movement. And if someone is rigid you can't have movement, so why bother to work with retarded people.

In those days, if you knew who the Kallikaks and Jukes were and you knew how to give a Stanford-Binet, you were an expert in mental retardation. But then in very quick order, we had an infusion of money and concern because of the Kennedy family. A lot of historical forces came together at that time, and we produced a number of very intriguing theories. There was the Zeaman attention work, Spitz's cortical satiation research, Luria's work in Russia, and on and on. I think most of those theories are wrong, but every theory is going to be wrong until the last person lives. That doesn't bother me. They were very exciting ideas. People were testing hypotheses that were theory-driven. I lived through that period, and it was wonderful. Those were the days, believe it or not, when people in Washington used to call you and say, "Hey, we've got money left over. Could you use a hundred thousand dollars?" I don't know if we did as well as we should have done at that time, but I've gone on too long. I've given you a twenty-year history which shows that things were very good, then they went sour. Are they going to turn good again? They'll only turn good to the extent that the entire field convinces decision-makers, "We really don't have the final answer. Let's continue to do some more work." That's been a theme of my talk today.

Q. One of the issues that we're seeing out in the field concerns many of the regular ed teachers who are involved in determining who goes into a special ed classroom or not. The Philadelphia public schools did a survey when they were seeing a ten-fold increase in the number of students being recommended for special ed, and they found primarily that the teachers were identifying these students based on behavior disorders. And the behavior disorder was basically disruptive of the learning process in the classroom. Whether they were in need of special services or not, the teacher determined they were interfering with what was going on instructionally. It seems to me that right now the key ingredient, and perhaps you can respond to this, is the regular ed classroom teacher, who in many cases would be more than happy to see special ed classrooms come back in vogue again. They are telling us that they are overwhelmed by the demands placed on them; they point to the special ed teacher as having extra training, extra sources, extra support; and they look back on their own classrooms, where they're given very little. It seems to me that the teachers that we're interacting with are being overwhelmed.

A. I think that a group that has had the most headaches as well as the most negative impact has been the teachers in mainstreamed classes. I did a study in several communities in Connecticut to test the fairly simple notion that how much training a teacher has had in working with handicapped children would be related to how well these children were doing on various psychological measures after being mainstreamed. The results were astonishing. What I discovered was that the median number of courses that these teachers had is one. Half had zero--no contact whatsoever. I put the blame in Washington. One of the things that always interests me about Washington, and I have been running back and forth to this town for a good long while, is how the left hand never knows what the right hand is doing. Let me illustrate how mainstreaming in the public schools might have been handled to make it work.

I was one of the planners of Head Start in this country. Then when I was in charge of Head Start in the early '70s, I discovered something that we hadn't even thought about in 1965 when we began the program. Handicapped children were not allowed in America's Head Start program between 1965 and 1971. And I was the decision-maker who said, "Why aren't we enrolling handicapped children in Head Start? We know that the prevalence is much higher among the poor than it is anywhere else." Once I announced that I was going to admit handicapped preschoolers, I heard a terrible outcry from Head Start teachers. "We're doing all we can do now. These children who are not handicapped but who are poor are a trial and a tribulation. Now you want us to deal with handicapped children. There's no possible way we can do more." Well, what we did is put into place a gigantic national support system for those teachers. We gave them special training on how to deal with these children. We gave them people who would come and work with them in the classes--support services. Today in Head Start, 12 to 13 percent of all the children are handicapped. And there are no longer loud outcries by the teachers that they're being overburdened.

No such support was offered when the Education for All Handicapped Children Act was passed. Schools were given ten thousand pieces of paper to fill out, but only 7 percent of the money that it really costs to deliver the services that were mandated. And nowhere in that whole package can I find any thought as to how to support a teacher who has never seen a mentally retarded child when suddenly one comes into her class. Some states have done better than others, but I think that's why you are seeing what you are seeing.

Assuming we have correct classification and careful diagnosis practices, I continue to be convinced that there are some children who are retarded and should be mainstreamed. It was always ridiculous for us to put children with I.Q.s of 70 or 65 in institutions, and some may not belong in special ed classes either. They really are just below the slow learners, as far as I'm concerned. But they do present special problems. You now have a more heterogeneous population to teach, which is always hellish for teachers. And those with behavior problems are like the proverbial squeaky wheels--they are going to get some grease. But what we are also seeing in our studies of mainstreamed children, which worries me, is the very quiet child whose adaptation is to simply blend in and never say a word. And of course, many teachers are happy to have such children in their classrooms since they create no problems. The real problem, however, is that the child just sits there, and time goes by, but that child is not being educated in any way.

Again, I go back to an experience we had in Sweden when Jim Gallagher and some other scholars and I went to look at what was going on there. The Swedish are really committed to normalization, and if a child fails in school the teachers think they've failed. My own analysis is that about half of the children in Sweden who are retarded by psychometric standards never get labeled as retarded. That would be a failure to the teacher. But what happens to those children? They get socially promoted grade by grade by grade, and when they graduate they find themselves unemployable.

As much as we're against labeling, and we all know the negative side of labeling, we often forget the positive side. My old colleague, Nick Hobbs, in his book on the futures of children, sets up a dictum that I like a lot, which is: "Never label a child unless the label carries with it enough positive services to offset the negative consequences of the labeling." The Swedish children who were spared the MR label were also spared the special services that would enable them to make it in their adult lives. Fortunately, unlike America, the Swedish government maintains a mental retardation registry and offers listed persons necessary services throughout their lives. So unlabeled graduates can still go to the registry and ask, "Would you please call me retarded so I can get training to make me employable?" So, support for teachers and the effects of labeling are the kinds of issues that continue to confound us in our excursion into mainstreaming.

Q. Mine is more in the nature of a comment also. (Part of question was inaudible.) How can colleges and universities better prepare teachers to keep students in classes, how can school systems support teachers so that everybody can stay in the regular classroom, and how can communities support and develop students within the community so that they can stay employed? (Rest of question was inaudible.)

A. You feel that way, but I ask you what is your ultimate criterion? I'm afraid too much of what we are advocating is based on what we would want for ourselves. If you look at the home for Prader-Willi individuals, run by the Vineland Training School in New Jersey, it has only P-W patients and some pretty restrictive policies. But those clients are doing much better. I think we have to re-evaluate the standards by which we say a practice is good or bad. To me, there are two issues. First, there is the issue of humane treatment. I don't need any research to say that what my old friend, Burt Blatt, took pictures of in institutions should never exist. There's a certain level of decent care that every human being has a right to. That's one issue. The second issue is that I don't think we ought to be making policy on what makes us feel good. It ought to be made on what makes the quality of life for retarded people as high as it can be as they see it, not as we see it. I think every human being has a level of functioning that can be optimized at any I.Q. level. So in my own work, I continue to look at certain characteristics that I know make for a more effective life. Those are my ultimate criteria for whether a practice is good or bad.

It's this kind of disagreement that my talk this morning was meant to bring about. I'm asking all of you to say, "Hey, why do you believe one thing rather than another thing? What's the evidence that you're using?" And as much as I respect your feelings, I don't think we can build a consensus on that basis, because everybody has different feelings. I would like to see our science based on something a little more empirical and data-driven than that. I think we should quit at this point.

M. Kaufman: Ed, thank you. The discussion that we're having, which is along the line of a standard of objectivity, going from an empirical line of objectivity to personal objectivity to getting consensus for the basis for objectivity, will thread itself through, I believe, the various discussions during the next day and a half. At this point we're scheduled for a break and then to your respective meeting rooms for the small groups. Thank you.

Naturalistic Inquiry: Politics and Implications for Special Education

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NATURALISTIC INQUIRY: POLITICS AND IMPLICATIONS FOR SPECIAL EDUCATION

It's a pleasure to be here, especially since some of my best friends are special educators. In fact, my guess is that one of my very best friends prompted this invitation. The shootout at the Dupont corral between Tom Skrtic, my old friend and colleague from Kansas and Robert Yin must have been great fun. For those of you who attended last year, this must seem the continuing episode of "naturalistic inquiry", which Egon and I are rather wont to call "Son of Naturalistic Inquiry", or "Bride of Naturalistic Inquiry". But since the request to talk concerns my favorite topic, I'm glad to do it.

Apparently Marty Kaufman felt that this group needed to know something in a more formal way about naturalistic inquiry, and I'd like to begin by laying some groundwork from the hard sciences. As most of you who read out of your discipline know, there has been a complete revolution in the hard sciences, a switch from the Cartesian-Newtonian worldview to something which I'll call the Heisenbergian universe. Since Heisenberg first enunciated his Uncertainty Principle following closely on Einstein's ruminations on the nature of matter and Schrodinger's famous metaphor of the cat who symbolized "created reality" -- physics has been operating at far remove from the classical physics which had predominated since the alchemists of the middle ages. The long-term effect of the Heisenberg Uncertainty Principle, Bell's Theorem, Einstein's theory of relativity, and Schrodinger's cat has been to release physics (and the rest of the hard sciences) from the logical positivist assumptions regarding whether or not objectivity is possible in science, whether or not there is a "real" reality out there, waiting to be converged upon, and whether generalization is possible in an a-historical sense.

The question as to whether the social and applied sciences can be more like the physical sciences is today a singularly interesting question. Those who ask it presume that science continues to operate along the lines of scientific method. Nothing could be farther from the truth, except in the most classical of studies. Science appears to be much more like Zen Buddhism (Zukav, 1979) than it appears to be Newtonian. Thus, my plea today is an unusual one. I would ask that you, like physics, chemistry, biology, and the other hard sciences, give some thought as to whether or not the conventional, Cartesian model of the universe is a serviceable one, or whether the particular constraints and problems of your own discipline - special education - might not be worth moving to a new vision of the universe.

Let me review what the old universe looked like. We call a unified theory of the universe a paradigm. A paradigm is simply a model which not only tells you what reality ought to be like, it tells you how to seek data from that reality, and how you ought to talk about the search for those data, or knowledge. The Newtonian universe was much like a clock, and in fact, the clockwork was a guiding metaphor for centuries. The mechanical, assembly-like properties of a clock led us to postulate that we could take apart reality into small chunks, study them one-by-one, and discern -- by building knowledge piece by piece -- how the entire clockwork worked. Assembly, machines, subsystems, aggregation, and determinism were a large part of this universal story regarding the cosmos. When set out in formal terms, the philosophical position was called logical positivism.

Essentially, logical positivism (depending on who you read) was composed of five axioms, usually subsumed today under the rubrics of ontology, epistemology, methodology and axiology. The five axioms run something like this, at least in their most rarified and idealized form.

1) reality -- reality was envisioned as a singular entity, subdivisible into pieces which would be studied independently of each other (we call them variables). It is "out there", and the purpose of science is to converge upon that reality until at last it can be described and understood. Science disagreed as to whether reality could be finally seen, or merely approximated, but no one disagreed as to whether or not it was really out there.

2) subject-object dualism -- the scientific or conventional view of the world understands that researchers and researched interact, all right, but believes that this is undesirable, since the role of the scientist is to be a disinterested observer, totally objective, uncontaminated by his or her phenomena. About the best than can be done is to put the thing under study at as much remove as possible, thereby bringing reactivity and

reactivity to a minimum. The ideal position, of course, is to have no interaction, as interaction introduces bias of the worst sort -- that is, untrackable bias -- into the experiment. The position on this epistemologically is to do the best you can, and hope for findings which are as contamination-proof as you can make them.

3) generalization -- the aim of science, since it must converge on that presumed reality out there, is to write laws which govern the reality. This worked for physical matter in the Newtonian universe (although please note, it does not work for subatomic particles), and John Stuart Mill believed it would work for the social world. If we just knew enough about social life, we could write laws, obey them, and engineer "Utopia". Fat chance. Nevertheless, the task of the scientist was to generate these time- and context-free laws, called generalizations, which would indicate the meta-laws under which nature and the social world operated. The very stuff of science was, finally, to be generalizations.

4) causality -- in a deterministic universe, nothing occurred without a prior, or at least temporally coterminous (called effective and efficient) cause. A causes B, or A, in the presence of B, brings about C. The thought-pattern is linear, determinism sets the world in motion, and the role of the scientist is to find out how things work

by describing the causes in the form of laws (those were the generalizations about which we spoke). Causality is spoken of in "if-then" statements, and once all the mechanisms and their efficient causes are described, we should be able to tell you how the universe works.

5) values -- science has been believed to be, and normatively hoped to be, value-free. Since science was conducted by disinterested observers, only the most partisan of scientists could be accused of embarking on value-laden research. The findings of science were taken to be pure knowledge about the social world, and therefore, without bias, prejudice or hidden values. When values did creep in, we labelled the scientist as "hardly objective" -- a terrible curse -- and dismissed his or her work as partisan, impure, and therefore, of no great moment or significance, certainly not in the policy formulation arena.

[INSERT TABLE 1 ABOUT HERE.]

These axioms are captured in Table 1.

Each of these specific axioms, of course, is a purist version, and we all know it. But until you see the real thing in pure form, you don't know what variations of it might exist. None of the axioms has been free from attack. Responsible scientists have always known that their inquiries were not exactly objective, and thoughtful ones have contemplated the role

TABLE 1

CONTRASTING POSITIVIST AND NATURALIST AXIOMS

Axioms About	Positivist Paradigm	Naturalist Paradigm
Ontology: The Nature of Reality.	Single, tangible, fragmentable, convergent.	Multiple, intangible, wholistic, divergent.
Objectivity: The Inquirer-Respondent Relationship.	Independent.	Inter-related.
Purpose: Generalization.	Context and time free generalizations; nomothetic statements; focus on similarities.	Context and time bound working hypotheses; idiographic statements; focus on differences.
Explanation: causality.	Real causes, temporally precedent or simultaneous.	Interactive shapers (feedback and feedforward).
Axiology: The Role of Values.	Value-free.	Value-bound.

of values in their choice of problems. So rare is the person who accepts these axioms wholesale. Nevertheless, they are useful to observe in their unmodified form, since they allow observation of a flawless logical positivist framework which should guide inquiry, even if it does not perform perfectly.

With what might you counter this system -- which has, after all, worked well for several hundred years ? You might play the geometry game. If you took Euclidian geometry and turned each axiom on its head, what would you have? You would have Lobachevskian geometry, a seemingly non-sensical geometry which could hardly be of use to anyone but another mathematician, right? Wrong. We need Lobachevskian geometry to put men on the moon and to recover them. Since Euclidian geometry is predicated on a linear, straight-line world, we cannot cope with the curvedness of outer space with that limitation; we need the predicated roundness of Lobachevskian geometry in order to plot circular and parabolic orbits, and get our astronauts safely home. I'd like to play the same game here, turning conventional, scientific, or logical positivist inquiry on its axiomatic head. If I did that, what would it look like The axioms would go something like this:

1) reality -- rather than a singular reality, reality would become a multiple, socially-constructed, divergent set of entities, theoretically endless, and at least as proliferated as there are persons who might hold different constructions. The more involved science becomes in a given question, the less it converges, and the more the research

diverges, like Portnoy's famous onion, layers upon layers of realities. Reality in this paradigm is built upon the assumption that it is created from moment to moment as various individuals enact it; it exists as persons experience their world: holistically, seamlessly. To attempt to tear it apart into something scientists call variables destroys essential elements of meaning hidden within, and does violence to the individual whose construction it is we are investigating.

2) subject-object dualism -- the essential reactivity and interactivity of human researchers and respondents is here recognized, but its treatment is conceptually, pedagogically and morally different from that of the conventional scientist's. The interactivity is not resented, nor is it presumed to be a matter of great methodological error potential. Rather, reactivity and interactivity are assumed to be opportunities for interdependent mutual learning, with respondent teaching researcher which questions are of high salience, and with researcher reflecting to respondent her or her understanding of contextual meaning, and the constructions of other persons. Researcher and researched each move between the roles of teacher and learner, teaching each other about the world they inhabit, and how they make meaning of those worlds.

Clearly, this is a research situation where the researcher cannot remain distanced from the object of her or his inquiry. Involvement is key, and that involvement has to have a very special quality. It must be honest, authentic, trustworthy, moral and utterly caring. Integrity in the research process is paramount with high face-to-face interaction between inquirer and respondents. (Please note that I have switched to the term respondents. The new paradigm demands that the use of the term "subjects", from the Latin _subjugo_, to go under the yoke, or to be enslaved, is wholly inappropriate. I prefer the term respondents, from the Latin _respondere_, to answer back as an equal. It should be apparent from this usage that I mean seriously to evoke a far more power-balanced form of inquiry than science has been accustomed to in the past, and indeed that is the case.)

3) generalization -- the possibility of nomothetic laws, time- and context-free rules about human behavior is non-existent in this form of inquiry. Since all human behavior is presumed to be time- and context-bound, and changing the time and the context may change the behavior --and, of course, the constructed reality --of the respondent, about the best you can hope for is idiographic and local knowledge (Geertz, 1983). This knowledge is encapsulated in what Lee Cronback calls "working hypotheses." Working hypotheses are propositions which have truth value for a given time and place; if one wants to know whether or not they hold somewhere else, that is a matter of

empirical testing and on-site verification, including comparison of sending and receiving contexts.

In this axiom, the social nature of the knowledge production function is recognized. Producing knowledge, however tentative it might be, is a form of human endeavor, complicated by history, time, place, and the belief systems of the co-producers.

4) causality -- clearly, in a non-deterministic universe, linear causal chains are insufficient to describe the complexity which characterizes human affairs. The alternative is to move to what Kaplan (1969) calls "pattern theories" of human behavior: theories where events and circumstances describe not linear chains, but rather patterns, much as a spider's web has a discernible pattern, even though each individual spider spins one differently. Events are viewed not from the perspective of straight-line order, but rather from a perspective of mutual influence, of plausible rather than definitive inferences, and not with variables, but with factor patternings. Conventional causality turns out to be as ephemeral and problematic as we intuitively knew it to be all along.

5) the role of values -- we are now in a position to acknowledge the role of values in the human enterprise known as science. Science cannot be value-free. Marion Namenwirth says that when scientists have declared they were not con-

scious of any bias in their research studies, this did not mean they were free of bias; it merely meant that they were unconscious (Namenwirth, 1987)!

We see now that values enter into the research process in at least five ways. First, values enter in when inquirers choose, frame and bound a problem. The choice of problem itself represents a set of value decisions, particularly regarding what individuals believe is important, or, what they believe is fundable. Second, values enter in when researchers choose a paradigm within which they will work. As Michael Patton points out, the choice of a paradigm is predominantly an unconscious act, handed down whole-cloth from one generation of inquirers to another. But in an age of the paradigm revolution, with two legitimate models for disciplined inquiry competing for primacy, the choice of paradigm is more problematic. You _must_ make a conscious choice, remembering Sister Carita's observation that to _not_ make a choice is to make a choice. After today, if not before, you will have had your consciousness raised, and if you do things the same way you have always done them, you will have made a decision, whether you recognize it or not.

Third, values enter in when inquirers choose an overall research strategy (which we call methodology) and a set of methods, qualitative or quantitative (which we call methods) to support the overall strategy. Fourth,

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> the inquirer faces values when she/he enters into a context and confronts respondents. Values inhere in contexts, and inquirers must take them into account. The choice of a context itself is a values decision. Working in the laboratory is a considerably different decision than working in a natural context.
>
> Finally, researchers are confronted with values when the previous decisions are compared. Comparing all the decisions for internal coherence and consonance allows one to make a judgment regarding whether all the previous decisions _support_ one another -- in which case, you may label the inquiry _resonant_, or whether the decisions are internally inconsistent or incongruent, in which case you label the inquiry _dissonant_. Values are an inescapable part of inquiry, just as they are an inescapable part of all human endeavors.

These axioms are displayed on the right side of Table 1, in contrast to the conventional axioms on the left side.

Rigor and Trustworthiness

There is a set of trustworthiness techniques developed to handle questions of rigor which might arise, although I'll not review them extensively here, since they are well-covered elsewhere (Lincoln and Guba, 1985). I do want to comment on them however. The conventional paradigm's criteria look like those in Table 2.

[INSERT TABLE 2 ABOUT HERE.]

We have substituted the terms credibility, plausibility, depen-

Egon G. Guba
August, 1980

TABLE 2.
THE SCIENTIFIC PARADIGM TREATMENT OF RIGOR

Inquiry can be affected by:*	Which produce effects of:	Design Criteria To guard against which we:	In the hope this action will lead to:	And produce findings that are:
Masking or competing factors	Confounding	Control and/or randomize**	Internal validity	Contamination-proof
Situational variations	Atypicality	Require probability sampling	External validity	Context-proof
Instrumental drift or decay	Instability	Replicate	Reliability	Inconsistency-proof
Investigator predilections	Bias	Insulate the investigator	Objectivity	Investigator-proof

* These factors are seen as introducing errors.

** Randomization is necessary in all cases because resource limitations prevent controlling all possible confounding variables. It is that fact that makes statistics so indispensible--to permit estimation of "error terms" (random effects) and testing of "real" effects against them for significance. But statistics require quantification. Is there a qualitative analog?

dability and confirmability for the conventional paradigm's rigor terminology of internal validity, external validity, reliability and objectivity. We have switched terms for several purposes: first, we wanted to indicate that new paradigms require new languages of discourse, and our terms represent a first start at a new discourse; we wanted to cue our audiences that conventional proofs of rigor were inappropriate; third, we wanted to indicate that when you are using primarily qualitative methods, as you would be here, you cannot expect to apply exactly the same criteria you might for "hard" or quantitative data. Field methods require different forms of testing for validity and reliability. The tests and internal structures are themselves in place; we have not invented something in the way of field methods which takes the place of anything else. We have freely borrowed and adapted from old field anthropologists their techniques for ensuring that the results of their research were authentic, that is, had a form of truth value, and therefore could be asserted to be something more than the product of a demented mind gone native. The criteria are displayed in Table 3.

[INSERT TABLE 3 ABOUT HERE]

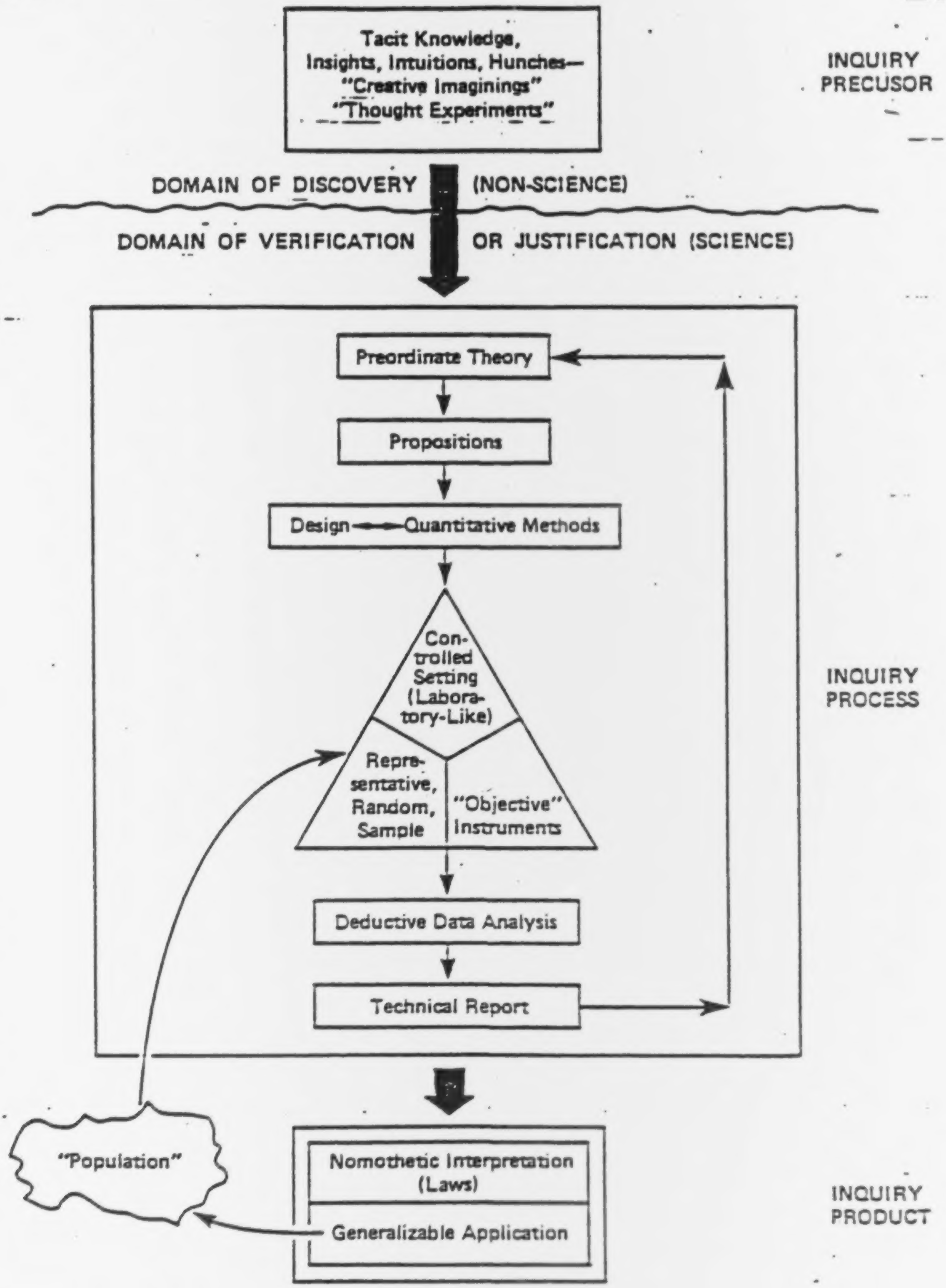
The important thing about all of this is not that you cannot get the same rigor in this paradigm that you believed you could in the conventional scientific paradigm. Conventional inquiry is a *closed system*. It walls off external criticism, non-corrorborating evidence, contrary data, in defense of the proposition under warrant. The model for conventional, or closed-system, inquiry looks like Figure 3. , And naturalistic

[INSERT FIGURE 1 ABOUT HERE.]

TABLE 3.
THE NATURALISTIC PARADIGM TREATMENT OF RIGOR

Inquiry can be affected by:	Which produce effects of:	Design Criteria To take account of which we:	In the hope this action will lead to:	And produce findings that are:
Factor patternings	Non-interpretability	Use prolonged engagement Use persistent observation Use debriefing by peers Use triangulation Establish structural corroboration Establish referential adequacy Do member checks	Credibility	Plausible
Situational uniquenesses	Non-comparability	Provide thick description Develop working hypotheses	Fittingness	Context-relevant
Instrumental changes	Instability	Use overlap method Use stepwise replication Leave audit trail	Auditability	Verifiable
Investigator predilections	Bias	Use triangulation	Confirmability	Investigator-free

FIGURE 1. THE REPRESENTATION OF CONVENTIONAL INQUIRY*


Tacit Knowledge,
Insights, Intuitions, Hunches—
"Creative Imaginings"
"Thought Experiments"
INQUIRY PRECUSOR
DOMAIN OF DISCOVERY (NON-SCIENCE)
DOMAIN OF VERIFICATION OR JUSTIFICATION (SCIENCE)
Preordinate Theory
Propositions
Design ↔ Quantitative Methods
Controlled Setting (Laboratory-Like)
Representative, Random, Sample
"Objective" Instruments
Deductive Data Analysis
Technical Report
INQUIRY PROCESS
"Population"
Nomothetic Interpretation (Laws)
Generalizable Application
INQUIRY PRODUCT

*All bounded by the problem, the evaluand, or the policy option, and tested by techniques relevant to positivistic trustworthiness criteria: internal validity, external validity, reliability, and objectivity.

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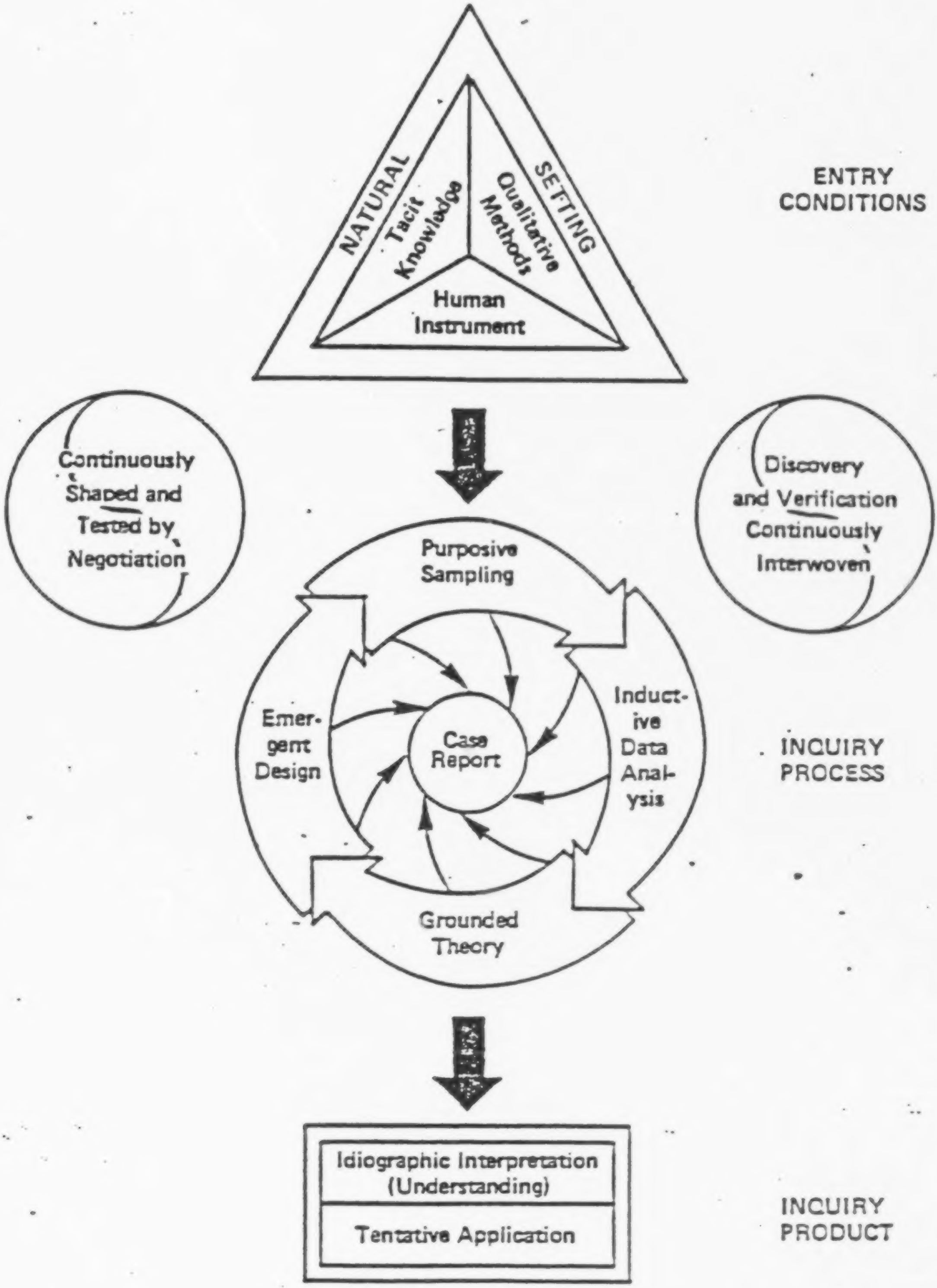
FIGURE 2.

THE REPRESENTATION OF NATURALISTIC INQUIRY*


NATURAL
SETTING
Tacit Knowledge
Qualitative Methods
Human Instrument
ENTRY CONDITIONS
Continuously Shaped and Tested by Negotiation
Discovery and Verification Continuously Interwoven
Purposive Sampling
Inductive Data Analysis
Grounded Theory
Emergent Design
Case Report
INQUIRY PROCESS
Idiographic Interpretation (Understanding)
Tentative Application
INQUIRY PRODUCT

*All bounded by the problem, evaluand, or policy option, and tested by techniques relevant to naturalistic trustworthiness criteria: credibility, transferability, dependability, and confirmability.

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inquiry is exactly the opposite: it is *open system* inquiry. As systems theorists among you may well remember, open systems are not impervious to outside data, outside evidence, or outside criticism. So there is a lack of finality, and a lack of elegance, to naturalistic inquiry. The lack of finality has to do with being open to new and contravening evidence at any moment; the lack of elegance has to do with the inquiry's reflection of the messy, multiple, social worlds of respondents. In short, when you switch, you are trading chic and smooth, the "Chanel effect", for rumpled and comfortable tweeds, the everyday-world, ordinary-language effect. You are trading your spit-shined wing-tip Florsheims for your Reeboks -- but at least they'll be *you*.

What are the Implications of New-Paradigm Inquiry for Special Education Researchers?

There are implications other than just axioms for what it is you do. Since this is open-systems inquiry, please note that your job has expanded some. Now you cannot just look for evidence. You must look for counter-evidence. You have to be committed to seeking constructions which are at variance with the ones you hold, or the ones which are held by a majority of your respondents. This is not something you do because you are a good researcher, although that is important. It is something you do because it is an ethical responsibility within the new paradigm, a moral obligation (please note here a new consideration: change paradigms and you change ethical problems and constraints). If there are mulitple constructions out there, you are under obligation to find them, to report them, to honor them. You are under obligation to have them enter into the negotiations regarding what gets into print about people, since they own their own data, and have a right to say how it is used.

And if you want their constructions, you must negotiate with them for those constructions. You cannot merely have them sign a consent form. They must understand what it is you're doing and why. And please notice, too, that deception, characteristic of many social and psychological studies in the past -- in the service of the search for that elusive "real" reality out there -- only thwarts the naturalist's search for multiple constructions. Thus deception is never warranted in the naturalistic, phenomenological paradigm. If you don't believe there is a single reality out there, but that reality is a social construction, then your job is to search for the multiple social constructions. You can't get at social constructions if your respondents don't know what to respond to. Thus the warrant for deceit is abrogated.

Closer to home, you have the problem of confronting what makes up your field. I would contend, with no disrespect intended, that most of the time, researchers do not know what makes up the field of special education. Special education researchers have exactly the same problem with giftedness or mental retardation or learning disability that the physicists have with the inside of an atom. We call it the "black box" syndrome. We cannot see into the brains of LD children, so we are left with the alternative of observing the outward process. How can we contend with that? Just as the physicists do: by making inferences from process as to what might be going on (which is, of course, what we have been doing all along -- although we should remember that science tells us this is a poor substitute for "real science"). The point of this is to help us see that we have been doing things in many ways all along which contravene pure scientific method. Now we can justify those sensible things with a formal philosophical stance.

Another implication is what I call the democratic option. Naturalism demands that inquirers treat respondents as they would like to be treated themselves: as persons with rights, with agency, and with the power to make many, if not most, of the decisions regarding their lives. This, of course, gets abbreviated somewhat when researchers deal with retarded persons, but this does not mean that rights can be abrogated because persons cannot speak for themselves. We have an obligation to bend over backward in helping our respondents understand what it is you need from them and how you will use this information. You have an obligation to tell them you are seeking constructions from others which may be in opposition to their own constructions. We have an obligation to not only ask for their information and data, but to check our research findings with them to discover whether or not they would agree with our interpretations of their realities.

In naturalistic inquiry, data cannot be, as they so often are in conventional inquiry, separated from the interpretations which grow from it. The interpretations are, after all, representations of the constructions which we have gathered. It is not enough to ask to use data; we must also request the use of interpretations -- ours, theirs, and those of others. And people have a right to remove more than their data; they have a right to demand that we do not present them in ways which they believe to be against their own best interests. We cannot hide behind our white lab coat and assert that what we are doing is for the larger purpose of gathering truth (translate: converging on that "real" reality out there), or serving society. Society is, after all, nothing more or less than a group of individuals who have given their common consent to live in lawful relations with one another, without violation of agreed-upon rights. The predominant purpose of naturalistic inquiry is furthering understanding -- understanding of how we group ourselves to achieve common purposes, understanding how patterns in society are sometimes good and sometimes impoverished, and understanding what sorts of things tend to occur together, without reference to causality. Verstehen: the Germans said it best when what they meant was profound insight and comprehension of something's essence.

On the Political Front

But there are other implications, too. We all live and work in a political world, a world where majority rule is the basis of the polity, and where legitimacy, primacy and hegemony often determine the "right". And the political implications of this form of inquiry are as important as the methodological implications. Just as the paradigm requires that we move out of the laboratory and into the natural setting, rely increasingly on the human as instrument, depend on qualitative methods as the best way to get at multiple social constructions, and let our research designs emerge as you begin to sense salient issues from the context rather than from our prior office-bound formulations, the paradigm also unfortunately requires that we make extraordinary justification for such work, that we will sometimes find ourselves looking extra hard for outlets to publish our research, that we will compete at a disadvantage for funding for our research.

This is because there is a revolution going on, and the conventional scientists, who currently hold hegemony, ascribe both primacy and legitimacy to one single paradigm, and only one form of discourse: the logical positivist stance. The question here is power -- power to control funding, power to control who is hired and who is promoted and tenured, power to determine what gets published, and power to influence policy decisions.

Darwin and Kuhn both believed that new paradigms can only succeed older ones when holders of the conventional die off or retire from faculties and research centers. I do not believe that is the case. There is a plethora of evidence from this field that members of the field are calling for an abjuration of the old paradigm. I believe we can change the way we do research in this generation.

But it will mean making concerted efforts to educate funding sources, program officers, journal editors, and deans about the utility, the purposes, the hoped-for products from non-conventional inquiry. It will mean that those of you in this audience, whom I am told represent the best, the brightest, the most powerful and the most senior, will have to put yourselves on the line. If you will not do this kind of research, at least you must support those who do as persons who may have a new vision of the world.

And finally, persons in the audience must rethink what it is that they are about. John Donne said, "No man is an island", and you are not, men and women, islands. We are historically-situated, socially-located beings, operating within dominant frames of reference of which we have little if any awareness. We have an obligation to become aware of those frames and social constructs. We are obliged to understand how science, like other political and social processes, has acted to preserve power in some and disenfranchise others, particularly women, persons of color, the elderly, the mentally disabled, the poor, children and those who have non-majority views of culture and society. We should understand how we speak from a position which is privileged, and therefore, legitimate; and when we do that, in our findings or our policy recommendations, we have a special obligation to speak for those who have no voice, and to write for those who have no outlet.

The important thing, from the perspective of today, about my being here is that we all understand that one paradigm reinforces power structures, and the competing paradigm reinforces democratic and participative modes of being in the world. Operating from one will preserve the status quo; operating from the other will necessarily redistribute the power balance. Just as science has political overtones and is not value-free so our own research processes have political overtones. How we will use that power, and on whose behalf, is up to us. As Werner Erhard is fond of saying, "Are you going to make a difference, or are you just going to run your racket?" A switch in paradigm is one way of making a difference. For sure what we've been doing so far hasn't.

THE END.

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Yvonna S. Lincoln -- NATURALISTIC INQUIRY: POLITICS AND IMPLICATIONS FOR SPECIAL EDUCATION

QUESTION & ANSWER SESSION

Q. I think that we've worked long and hard the last couple of years, at our university at least, to try to make room for and to allow qualitative forms of investigations to occur, and that we're looking for accommodation in viewing them as different forms of research and methodology rather than competing forms. That's something that I think we've tried at our place, and I'm wondering what other folks have seen in their own institutions. It may not be an issue of either/or; it may be an issue of collaboration. It seems that in our own institution, there are more and more studies that combine qualitative and quantitative approaches. Statisticians (meaning myself) sometimes have a difficult time explaining significance. And the qualitative researchers, on the other hand, can add some dimension to the actual results that we are getting and the framework that we're working in. So, I think it's important, instead of looking at either/or, to look at how those two can collaborate.

A. This is often suggested, but please notice that is not a switch in paradigm. That is merely a switch in methods, and that's certainly one stance on the paradigm revolution, which is "We don't need to fix logical positivism, all we have to do is use more qualitative methods." I don't have any problem with that, but you need to understand that we're talking at very different levels of the onion.

Q. I guess it's a question, too, of whether you're talking evolution or revolution, and I think more people are comfortable with evolution than they are with revolution. And if you're going to change, (viewing change as a process), then I think you're going to see more of that initially than people simply switching from one domain to the other.

A. I don't have any trouble with that interpretation, too. I think most people do not like dramatic change. No doubt about it. I, myself, think 400 years is hardly a revolution. I think it is sort of evolutionary. We waited a long time, but that's O.K.

Q. We're talking about a paradigmatic shift, and yet this approach to investigation has existed for decades. Aren't we actually seeing it applied in areas in which it wasn't applied before, rather than a paradigmatic shift?

A. You're right on both points. This is not new. What strikes me as new is that this is the first time anybody has created some kind of whole system for a phenomenologically based world view. Yes, it has existed for a long time. And it's absolutely true that many applied social sciences have used it. The question is one of primacy. It does not have great legitimacy now, and the problems that I've pointed out I have case studies to document.

People can't get funded because they want to do this kind of inquiry. So it's not that it hasn't been around, it's not that it's not well recognized in other social sciences. It's just that in education, educational research, and I'm using it in a very broad way, we simply have not given it much credence.

Q. Those of us who have done single-subject research have encountered exactly the same thing in the last 25 years, and interestingly enough, in the last 10 years that these people have gone back and found incidences or examples that demonstrate such approaches have been used. So, it seems to me that you're talking about the social phenomenon of acceptability in approaches. I don't know if you can cut it short, but I think that (rest of sentence inaudible).

A. But twenty years is better than waiting for everybody to die off, right?

Q. Can you give examples of educational research where this kind of approach has been used successfully?

A. I can't cite you findings, but, of course, Tom Skrtic's technical report exists in the ERIC database--it's absolutely excellent. It has a set of case studies, it has a set of policy recommendations, and it has a very extensive technical report that tells you how the field work was carried out and in what ways it's internally consistent with the axioms of naturalistic inquiry. It's well worth the investment of the sixty bucks or whatever it takes to pull it out of the ERIC system--it's four volumes long, but it's absolutely first-rate. And it's in your area of special education. It's called Special Education in Rural America by Skrtic, Guba and Knowlton. It's the best full length, two-and-a-half year study that I know about. And that would probably be much more consonant with some of your projects. So it looks like your research.

Q. (First part of question inaudible) and the follow up to that would be, is acceptance of the paradigm more than an article of faith, and are there problems with the frameworks we already have not tolerating the tenets that we have here?

A. You cannot, without going crazy, both believe that there is a reality and there isn't a reality all at the same time; that generalization is possible, and generalization is impossible; that linear causality is possible, and linear causality is impossible; and keep all of that stuff in your mind, just like the queen in Alice in Wonderland, remember, who believed three impossible things before breakfast every day. You're welcome to blend them, but remember, we're talking here about world views, we're talking about what you think the nature of the cosmos is. And my guess is--as Egon is wont to say, "You're going to go crazy trying to believe this stuff." You can't mix and match, for the simple reason that the axioms are mutually reinforcing of one another. If you believe there is a reality out there, but that you can write no generalization about it, then who would believe there is a reality out there? See what I'm saying? You can't blend them very well. It's like saying to yourself, "I believe there's a God, but I don't believe there's a God." To take both of those propositions seriously, to really try to believe those at once, will drive you nuts. So, no, you can't blend the paradigms. I remember now what your second choice was: Koons says that the paradigm switch is not a matter of scientific proofs, that it's a conversion experience, an article of faith. He says you just decide that one of the other feels more comfortable, or matches your own particular experience. That's essentially how you decide what your paradigm is.

A. A follow up, if I may. Do you think it's important to know what is the paradigm of those who are your respondents? And is it your experience that the respondents are generally more knowledgeable about conventional science?

Q. That's a question for which I have no answer. I think if you're doing very close, careful interviewing, you will come to know aspects of the world view of your respondents. I don't think that most of the people that you use as respondents will be able to articulate it in a nice clear, clean way--the way we've done here today--to say this one and this is the other and these are all the tenets of my faith. That's too much like The Book of Common Prayer. Most people can't do that. But if you're doing good interviewing or good observation, you can make inferences about the meanings that people attach--it depends upon your problem. You had another question, too.

Q. Where can we read about the paradigm shifts in the hard sciences, if we're really interested in looking at how the shift is functional in the hard sciences?

A. Probably the best thing that's available is in paperback, and it's by Gary Zukav. It's called The Dancing Wu-Li Masters, believe it or not. And what it does is look at the new physics. It gives you sort of a history, and Zukav is not a scientist--he's a technical reporter for a newspaper. He went about interviewing physicists, saying, "What is this new physics that you're doing, because I don't understand it." It's told in lay-person's terms--you don't have to be a physicist to understand this--and you can buy the book for $2.95 on the newsstand. It's still in print. Fritzof Capra is a good one on the paradigm shift. The new book, Chaos, is one. Chemistry Transformed, by Charles McCann, which talks about the paradigm revolution when chemistry switched from phlogiston to oxygen. The French scientists were saying, "O.K., we can understand this. This oxygen is cool and it accounts for all of the phenomena we have observed for centuries, but explain to us what happened to the phlogiston." That's kind of what people who are in the middle of a paradigm shift do. They're O.K with oxygen, just tell them where the other stuff went. Those are all places to read about it, plus Time and Newsweek magazines, the latest issue on Stephen Hawking, the physicist who's trying to unify macro- and micro-physics into a grand cosmos theory. If you can get through that stuff, you're well on your way to understanding what's happening in the hard sciences. Actually, it's quite interesting. It's an adventure into the hard sciences, and Zukav says (I hate to use this word) it's "psychedelic" what's happening out there. But he explains things like Heisenberg's uncertainty principle and Bell's theorem and how relativity enters into this. And he explains what Schrodinger's cat is, and what it means, in lay-person's terms. Excellent book.

Q. I find your predicates provocative, ones that many of us share, but I don't necessarily think that your conclusions follow.

A. Where do you take exception?

Q. Well, one, perhaps in the characterization of what the current state of thinking about the philosophy of science is in a group like this. I get a feeling that I'm hearing about 1940's kind of logical positivism, rather than a contemporary version of science as it occurs in purely conventional forms, and kind of a work-a-day operational look. It seems to me that we've incorporated many different aspects from chronological approaches, we've read our Gestalt. Well, we have a lot of different things (rest of sentence inaudible). So I disagree with your conclusion that we must have a revolution. It seems to me that what we've learned from the physical sciences is that we didn't reject Newtonian physics when Einstein gave us a different view; we said there are certain classes of problems for which the one view serves us well and other classes of problems for which another view would be more profitable. Now, when you argue that we must reject the one in order to accept the other, that's good scientific theatre, but I think that it runs the risk of not paying careful enough attention to some of the basics by which every science has "boot-strapped" itself. We need decent descriptions of environments in a variety of ways. So, it is in the conclusion that no, not evolution, up with revolution, that I think your case is a little overstated.

A. I think you haven't been in the literature. I'm currently working on an extraordinarily long piece; I have reviewed nearly a thousand pieces of literature from 30 different academic disiciplines. We have a revolution. Nobody is going to revolt based on whatever Yvonna Lincoln says. Who's Yvonna Lincoln? I know four people in the audience. Right? People are in revolt. The academic disciplines are in open revolt, and they have been for more than a dozen years. My call to you is to be a part of the revolution, not to call for a revolution. We will, we are evolving. My call is not to say you must decide if you are going to have a forcible revolution or have a nice gentle change-process-oriented evolution. There is a revolution going on out there, and it does have to with the phenomenological paradigm. Now, with respect to your other question, I think that's right, but that's why I put caveats on the early part of my statement to say, "These things up here on the left side of the chart represent, essentially, very rarified forms of conventional inquiry." And I don't think you, or anybody else, operates by those. But I do believe that they do represent assumptions about the nature of the universe. I think they do represent the operating assumptions. For instance, throw me any six articles out of your journal, and I'll do a content analysis for you, and show you where the assumptions exist. That they are routinely violated we all know. I gave you that on page four.

Q. Will you specify the rules in advance?

A. Pardon?

Q. What are the rules by which you will do that?

A. But you see, the point is that individual researchers do it. I don't understand the thrust of what you're saying. My comment to you would be, individual researchers make up the rules about what they will systematically violate as they go along. I can't tell you what those rules are. I can tell you about conversations with different researchers in this room and outside of this room, and I can tell you what they say to me. They say things like, "Well, of course, we know that this isn't 'objective,' but we have to talk that kind of language if we want to get funded." I think individual researchers and teams make up the rules for where and under what conditions they'll stretch the belief system. I'll give you that. I don't have any problem with that. But it doesn't "fix" the system.

Q. To say that there is a group of people that you've had a conversation with, with whom there is an admission that the process isn't working well, and that therefore the process is not adequate on the basis on that line of reasoning, does not really discredit what is the system.

A. I think we've been miscommunicating because I didn't reason in particularly that way. I reasoned by a content analysis from the literature out there, not because individual researchers told me. Individual researchers acted as qualitative data sources for what we saw going on in the literature. I'm not sure that's how I would characterize an ad hominem attack, or argument, or whatever. In any event, they were not my primary or sole data sources.

Q. Looking through the literature, where those are your respondents and you're looking at their assumptions, vis-a-vis naturalistic or rationalistic explanations, how do you check your interpretations when you're talking about thousands and thousands of articles? How do you go back, from the naturalistic ethical perspective, to see if, in fact, they agree that your characterization of their methods is accurate?

A. Sometimes you ask, but sometimes you don't have to--they just say it. They say we have found three causes...

Q. You mean it's written?

A. Yes, it's written in a journal article. They assert it in technical language.

Q. O.K. I thought it was more of a dialog process--that you were characterizing it from that as well as the (inaudible).

A. Only with my doctoral students.

Q. They're making another strawman that you've set up but haven't acknowledged, and that has to do with the difference between the single, isolated researcher and the research community. And I want you to make a speech as to how findings in naturalistic inquiry accrue from study to study. I have an image of our fine state of unity as being one that has within it individual studies which are all flawed, and all violate assumptions, and all are poorly designed. But in toto, they begin to approximate something like truths. Maybe the question is not even apropros in the new jargon, but if knowledge is so local, how does knowledge improve from study to study?

A. You heard that question? This is one of the funner questions around, because it starts from an assumption that the way we build knowledge is exactly the way Egyptians built pyramids. You know, we put blocks, and then we put blocks on top of those, and when we get up here you should have what Stephen Hawking's wants, which is the unified theory of the universe. All you have to do is change your social construction about what knowledge is. See, I don't think that all knowledge (I think there is some knowledge) is knowledge that we can aggregate and treat taxonomically. I think there are other kinds of knowledge which may be circular--non-linear, non-hierarchical, non-parametal. So, the question about how do we know if knowledge accrues, becomes a critical one. It's actually a very good question. I'm making light of it because it's sort of warm up here and I'm getting kind of ditsy, but if there are some knowledges which do not accrue, that is aggregate, pile up, stack up, look neat and square, then what constitutes knowledge? And my response would be, ever-increasing sophistication and understanding about social and human processes--that's not necessarily an aggregationist statement. Do you see what I mean? I don't know where to go. I don't have all the answers, but that's my best answer now. I honestly believe there are some knowledges that we have which are not accretionary, which do not accrue, in the same way interest accrues on your checking account. I think there's some knowledge which enlarges, there are some bodies of knowledge, maybe, which address impoverishment, knowledge impoverishment, or spiritual impoverishment, and I believe that those knowledges may be circular, they may be spiral, they may be helical. They represent not bits and pieces that we can put on a pile somewhere as what science has done, but they represent increased sophistication among all of us about what is happening and what we want to do about what is happening. I think, in other words, that your model is a singular model, but it's only one model of knowledge. And that it is a helpful model of knowledge, but it's not by a long shot all the models of knowledge which we could be using.

Q. One of the problems that I'm experiencing is that you have (words inaudible) a very extensive and intricate presentation, that it's not really possible to think through and respond to (words inaudible) but that's part of the structural problem. I've read the Capra book quite closely, (rest of question inaudible).

A. You need to read the book or see the movie, Naturalistic Inquiry. I couldn't begin to do in 35 minutes any kind of justice to the kind of arguments that are mounted. You still might not buy into the arguments. You might never want to be converted, but at least you could see the form and the structure that the arguments take. I think that's what you're calling for here, a chance to be able to chew some of this stuff over and think about it and react to it. And it might help you if you picked up the book because then you've got something much more substantive. You're right, you don't have anything in your hands at the moment, to work with. The arguments I've made today in more extended and substantive form would be helpful.

Panel Discussion: What is Effectiveness?

Moderator: Naomi Zigmond

Panelists: Phillip S. Strain
Eugene Edgar
Joseph Jenkins

Panel Discussion:

What is Effectiveness?

Naomi Zigmond, Moderator: One of the most critical questions facing practitioners and researchers in special education concerns the definition of effectiveness. What is special education for? What are indicators that special education services have been effective? Should we, as a field, be satisfied with very narrow definitions of effectiveness, i.e., positive changes in a single behavior which has been the target of instruction? Or, do we expect special education services, especially those designed for mildly handicapped student to remedy the students' problems, make the students all better? We have all faced this issue, as practitioners and as researchers: for example, a master's student of mine, after some years out in the field, came back and told me about her experience with learning disabled students. She had worked *relentlessly* to have these students achieve some success in academic work, and at the end of a couple of years, most of her students had done remarkably well. When she proudly displayed the data to her supervisor, the supervisor said, "Well, they probably weren't learning disabled to begin with!" Clearly, the supervisor's view of effectiveness was not that the students would be "cured." If a teacher could accomplish that, the students must have been misdiagnosed!

The issue came up again in some of the work that Helen Thornton and I have been doing on dropouts. We locate high school earning disabled graduates and dropouts a year or so after what should have been 12th grade. We ask these young people to take a basic skills test and we inquire about their employment status and employment history. When we look at the basic skills levels of learning disabled students who have graduated from high school, we find that they are still very far inferior to the levels of control peers. Does that mean that the special education program given to these LD students had not been effective? Should we have expected special education services to narrow the achievement gap? On the other hand, the LD graduates were employed at rates that were equal to, and at pay rates that were equal to, non-disabled peers who had also graduated from high school. Did that mean that their special education program had actually been a success? Should we expect that there would be some life-long penalty for being learning disabled? After all, LD was a condition these young people carried with them even into the employment market. Or are *effective* special education services ones that produce no long-term penalty?

Well, I brought these sort of ill-formed questions about how to measure the effectiveness of special education to the Planning Committee last December, and the outcome was this panel this morning: a discussion of "effectiveness" in special education, and more specifically, since many of us are involved in intervention research, a discussion of appropriate measures of successful interventions, i.e. appropriate measures of effectiveness? We have with us Phil Strain from the University of Pittsburgh, Joe Jenkins from the University of Washington, and Eugene Edgar from the University of Washington. Each one, in turn, will give us their views on defining the outcomes of special education, one at pre-school, elementary, and secondary and post-secondary levels. They will address what special education trying to accomplish, and how do we measure that? We're going to go in my favorite order, reverse alphabetical, which means we start with Strain, and then Jenkins, and then Edgar. We'll hold questions until the end when I hope we will have time for an interactive discussion.

Some Thoughts on Effective Intervention for Handicapped Preschoolers

Phillip S. Strain, Ph.D.
Western Psychiatric Institute and Clinic
University of Pittsburgh School of Medicine
Pittsburgh, Pennsylvania

Paper presented at 1988 OSEP Research Project Directors' Conference, Washington, D.C., July, 1988

I want to begin by pointing out that being effective, as in helping children to learn important things about themselves and the world around them, is a small piece of what it takes to move empirical findings from the literary museums we call journals to everyday practice. We in the research community have a much more broad, difficult, and occasionally improbable agenda. As indicated in Figure 1, we must also be efficient, economical,

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Insert Figure 1 Here

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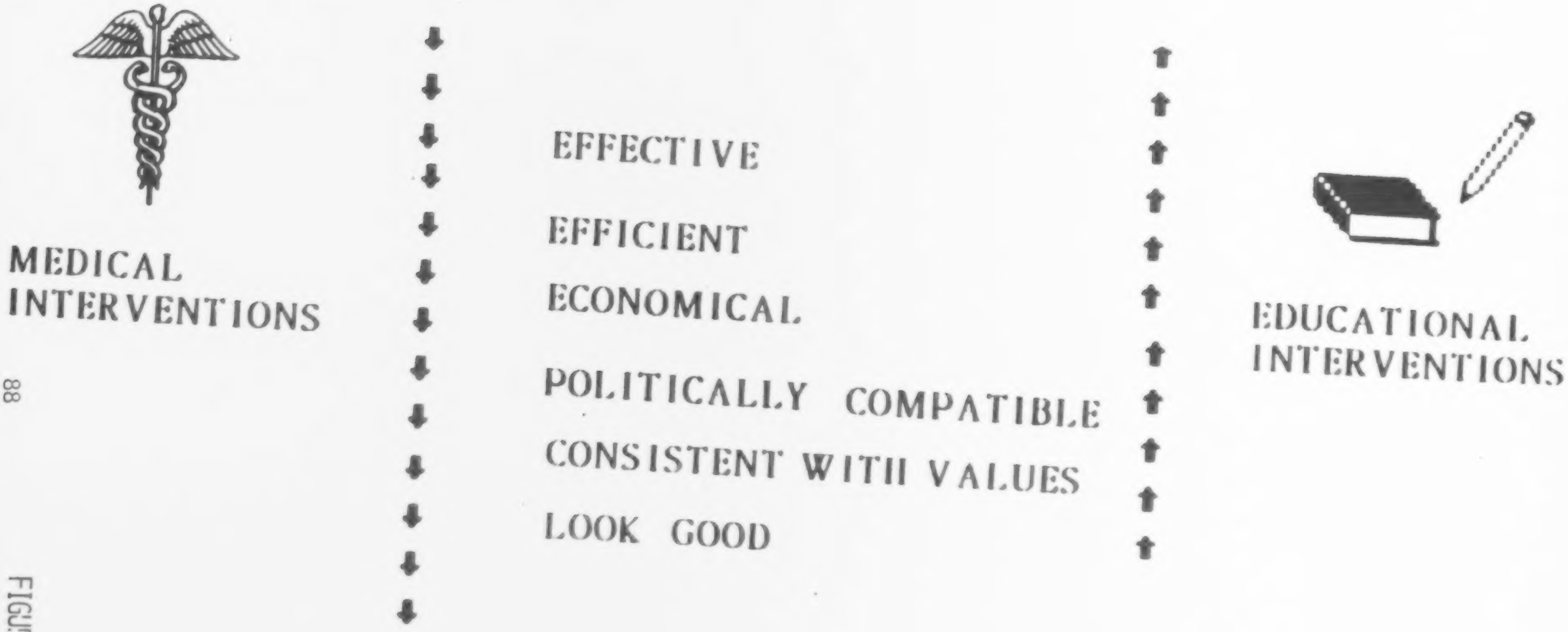
politically compatible, consistent with values and, of course, we have to look to the outside world.

I have also tried to suggest in this Figure that the adoption of educational practices often follows a path that belatedly considers effectiveness. And, I have contrasted this path with a more typical one for medical procedures, where effectiveness is always the initial concern. Finally, let me suggest that many educational researchers see the world quite differently from those who will or will not adopt their effective procedures. We have, I think, made some honest attempts in recent years to be efficient, economical, compatible, and all the rest. I think we should continue to pursue that course, with this caveat: That we nail the question of effectiveness prior to working on other standards for adoption. I also think we should go about studying how to make effectiveness a more valued dimension in the eyes of classroom teachers, school administrators, and tax payers.



MEDICAL
INTERVENTIONS

EFFECTIVE

EFFICIENT

ECONOMICAL

POLITICALLY COMPATIBLE

CONSISTENT WITH VALUES

LOOK GOOD

EDUCATIONAL
INTERVENTIONS

FIGURE 1

The second point I would like to raise is very much related to the first. The point has to do with using empirical methods to understand how our various consumers evaluate effectiveness. Figure 2 depicts some of the relevant concerns around consumer issues. At the most basic level we need to know if the opinions of consumers are positive, neutral, or negative in regard to effective intervention practices. We know enough at this point to say that effective interventions will not be used if people do not like them, or, if they cannot see a difference in child outcomes attributable to these interventions. It is interesting to me that this phenomenon is so widespread. It applies to the use of aversive procedures to treat self-injury, the full litany of behavior management procedures applied to behavior problem children, self-monitoring with adolescent offenders, and integrated service delivery for preschoolers.

Insert Figure 2 Here

At a slightly more complex level we need to determine the relationship between opinions and specific outcomes. For example, does a unit of child behavior change yield a unit of parent or teacher satisfaction? Do we have to produce the behavior change for a long time for people to see it? What are people really happy about? Is it child behavior change or the intervention experience itself? Of course, to answer any of these questions we have to decide that what people say about our interventions is important and valid. If we launch such studies

HOW CONSUMERS EVALUATE EFFECTIVENESS

1. Are the opinions positive, neutral, or negative?

2. What is the relationship between opinions and specific behavioral outcomes?

3. What is the threshold of behavior change that is perceptible to consumers?

FIGURE 2

to answer these questions on a wide-scale basis then we might eventually reach the final goal of identifying levels of child behavior change that predict good consumer opinions, and therefore predict a good likelihood of continued intervention use.

My final point regarding effectiveness reflects a strange form of amnesia that overcomes researchers of every theoretical persuasion who are fortunate enough to have any positive, long-term follow-up data. We forget about the contemporary environment while giving superordinate credit to historical events. I think this forgetfullness is a problem because it obscures an analysis of why we continue to be effective, or not. In early childhood work at least, what happened as much as 20 years ago is somehow directly causative of the most complex, interactive, and elusive of human behavior. As a mild aversive, let me point out that the only theory which is consistent with such a historical attribution is psychoanalytic, pre Anna Freud. While the interactionists may argue that they have derived a conceptually believable scheme to handle two decades of person-environment transitions, there are too many unknown steps in the sequence to make me very comfortable with such an approach.

As an alternative to looking solely to the past, or to intervening and as yet non-specified events, let me offer a simple minded example of how follow-up data can be considered as influenced by the contemporaneous environment; influenced by, therefore controllable by, and predictable.

Consider the illustrative data in Figure 3. Each panel represents a different grade level (K-2) and each data point within panels represents one days' data collection. The relationship between on-task behavior on the part of the former early intervention recipient and class structure is suggestive, and these correlational findings set the stage for the subsequent functional analysis of on-task behavior as a follow-up measure.

Insert Figure 3 Here

I am not suggesting in this Figure that all follow-up measures are so fluid. I am suggesting, however, that one need not rely solely on history to explain variance in follow-up data. More importantly, I am suggesting the possibility that "being effective" over the long-haul may well imply longitudinal intervention. Trying to define effectiveness is a bit like trying to throw a fastball soaked in 40-weight oil. The harder you try, the less likely it seems that success will come your way. At least part of the fastball and definitional problems come, I think, from persuing a well-travelled and fruitless road one too many times. We need a rather radical set of alternatives. Try as we might, we cannot throw our fastball soaked in oil and we can not grasp effectiveness with a singular focus on immediate child outcomes. Our focus also needs to include longitudinal outcomes and a careful analysis of contemporaneous environments. Our focus also needs to include the opinions of consumers, sampled rigorously and analyzed

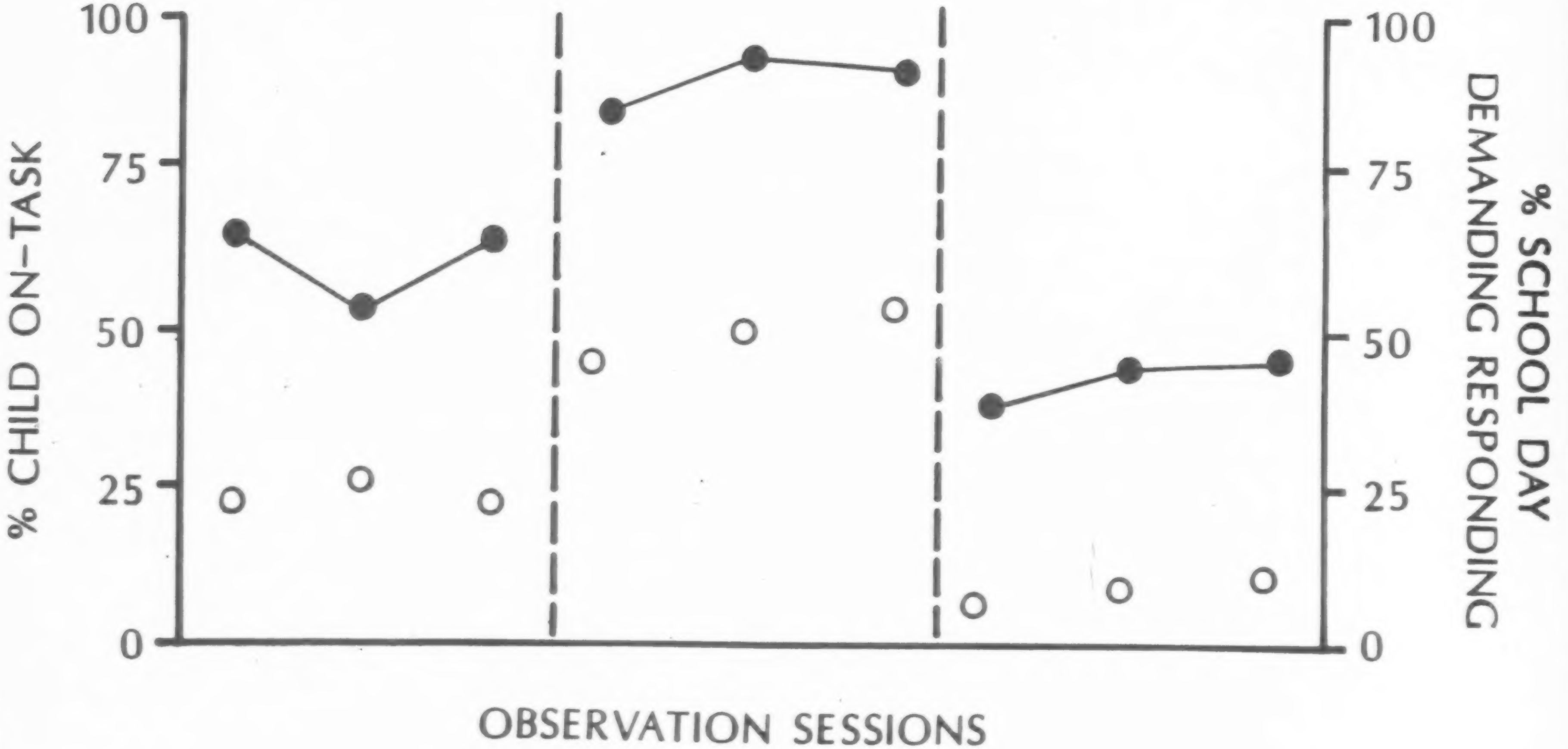

100
75
50
25
0
% CHILD ON-TASK
OBSERVATION SESSIONS
100
75
50
25
0
% SCHOOL DAY
DEMANDING RESPONDING

FIGURE 3

accordingly. Finally, our focus needs to include the full-range of dimensions that influence school practices and child outcomes. Efficient, economical, and politically compatible are a few of the other adjectives that must apply to our intervention procedures.

Markers of Effectiveness at the
Secondary Level in Special Education

Eugene Edgar
University of Washington

July 12, 1988
Annual Research Project Directors Meeting
Washington, DC

There is a point on the Willamette River near Eugene, Oregon, where a classical Oregonianism is to be found: "Warning, dangerous rapids ahead, only expert kayakers or fools should proceed." Attempting to discuss the effectiveness of secondary and postsecondary programs in special education is no easy matter. Even developing a framework by which to conceptualize an evaluation schema is fraught with philosophical rapids and methodological whirlpools. To venture past this initial point of conjecture requires expertise that exceeds my current white water rating. However, never being one who lacks foolhardiness, I will slide on by this starting point.

Following Naomi's lead, I have organized my thoughts around her four basic questions:

1) What is special education trying to accomplish?
2) How can these outcomes be measured?
3) What are the indications that special education programs have been effective?
4) From a research perspective, what are the "rules of evidence" to support a contention that special education is effective?

What is Special Education Trying to Accomplish?

This is obviously the most crucial point of the entire exercise. What is the purpose of special education, or indeed of public education in general? The easy answer for us today is to accept the "OSERS Bridge" model and declare that special education is "an outcome oriented process encompassing a broad array of services and experiences that lead to employment" (Will, 1984, p. 1). I personally reject that premise as being far too simplistic.

Chester Finn (current Assistant Secretary for Research and Improvement in the Department of Education) views the outcomes of schools in terms of preparing students (all students, he argues) with skills for 1) the social system in which we live, 2) personally fulfilling lives, and 3) the next phase of their lives, be it employment or higher education (Finn, 1986).

A third view is that of Wehlage (1983), who has developed proposed outcomes based on adolescent developmental theory. He advocates teaching coping skills (self-management, conflict resolution, and problem solving) as well as specific skills.

Thus, we desire our students to be job-ready for employment, knowledgable of our political system, self-assured as to "who they are," competent in reading, socially adept, emotionally intact...truly ready to be productive contributors to our society.

These definitions of desired outcomes ring more true to me than simply advocating for employment. But this stretch of white water is lethal. Philosophically, we (in the United States) view the public education system (K-12, or 0-21 for us in special education) as the "great entitlement," the process by which each of us is given the opportunity to partake in our way of life. Education is the religion of democracy (Boorstin, 1974). The converse is that, other than the public school system, our society provides scant assistance to its citizens. Public schools are "it" as far as opportunities go for receiving services from our government (entitlements, assistance, help, compassion). For many of our citizens, and especially those from the underclass, public schools are their only chance to acquire a reasonable shot at having a minimally acceptable quality of life. Given this enormous importance of public education, the debate on desired outcomes for special education students at the secondary level must not stop with easy solutions such as "employment" or esoteric cliches such as "productive contributors to our society."

I contend that the important aspect of secondary special education is to prepare our students with skills and attitudes that will enable them to experience some quality of life (have choices, experience joy, interact with friends, feel productive, care for their personal needs, compete in our society). I also believe the outcome of schooling should include opportunities to practice these skills in the natural environment. Thus, experience is also an outcome. I also believe that the education system must assume the role of identifying those students who will require ongoing support and assist those individuals in locating needed services. Finally, I believe educational professionals have the ethical duty to inform their fellow citizens of the human service needs of their students.

Now, I am the first to acknowledge that my list is no better (nor worse, I will add) than those proposed earlier. This is a thorny issue (how many metaphors can I use?). My final recommendation is that this debate become a valued part of our "scientific endeavors." I know, and agree to a large extent, that we need to base our discussions on data, that our journal articles need to be data-based, empirical inquires into the nature of nature herself. Yet, are we asking the right question?

> "It is axiomatic in science that progress hinges on asking the right question. Surprisingly, once the right question is asked the answer seems almost to tumble forth. That is a retrospective view; in prospect, it takes genuine (and mysterious) insight to see correctly into the brambles created by previous ill-chosen verbalizations" (Hardin, 1978, p. 29).

We, as a subgroup of our profession, should advocate for open debate on "the purpose of special education."

How Can These Outcomes Be Measured?

How does one measure quality of life? Or the presence of skills and attitudes necessary to freely partake of "our way of life" without resorting to concrete, objective facts such as salary level and place of residence, or subjective self-report statements ("I'm satisfied with my life")? These are difficult questions we must all confront and debate.

Conducting follow-up studies of special education school leavers poses many problems. Even after the outcomes have been defined, a major issue remains: who provides the information---former students, parents, a third party? For some questions, the answer is clear---i.e., How satisfied are you with your life? For others, however, there is no clear method---i.e., What is your child's current employment status? Sometimes the parents will not know the employment status of their adult child, sometimes the former student will not tell the truth, and locating knowledgeable third-party informants is often very difficult. In other instances, the former student is not able to communicate. Selecting informants is clearly a difficult task.

Another issue to consider is how to obtain the data. The most cost-effective procedure is the telephone interview. This practice may not be as efficient as in-person interviews where cost, however, is very high. Written questionnaires seem least desirable and often result in a biased sample as well as a low return rate.

When to probe is also an issue. Most studies report data sometime during the first year after leaving high school. However, we need more data about the lives of former students during the years after exit. Just how long to track these students is not clear. There is some evidence that all American youth "flounder" for the first several years after high school (Hamilton, 1986). If this is true we need to follow our graduates for 3-4 years after graduation mainly to determine their eventual status in life.

Finally, there is the issue of quality. Regardless of how many minutes I spend thinking about how to measure quality, I always return to ethnographic procedures. The work of Andrea Zetlin and Mike Murtaugh (1987) provides examples of the type of data that can be obtained using these procedures. Problems with small Ns and reliability of measures notwithstanding, ethnographic methods yield information on quality issues (i.e., type of friendships, extent of opportunities) that can be obtained in no other way.

What Are The Indications that Special Education Programs Have Been Effective?

There have been numerous follow-up studies of special education school leavers (graduates, age-outs, and dropouts) conducted in the 1980s. These studies have used parents or students as informants, have sampled students at 1-5 years from the point of leaving school, and have focused almost entirely on job status, postsecondary education, and living situation. There is a fairly consistent pattern of results.

Overall, approximately 60% of the special education graduates are employed (Mithaug, Horiuchi, & Fanning, 1985). For students with more severe disabilities, such as moderate retardation, the employment rate is lower (41%) (Wehman, Kregel,

& Seyfaith, 1985), while close to 70% of the LD students are employed (Zigmond & Thornton, 1985).

The data vary considerably in regard to postsecondary education. Hearing and visually impaired students have a high rate of attendance (60%) as compared to severely behavior disabled students (23%) (Edgar & Levine, 1987). The real test of postsecondary attendance, of course, is graduation rates, for which there are few data.

Most special education graduates live with their parents or relatives 2-3 years after leaving high school. Of course, this is also true of nondisabled youth.

In addition, about 25% of all special education school leavers tend to earn more than the minimum wage and few receive any type of benefits (i.e., health insurance) (Hasazi, Johnson, Gordon & Hull, 1988). The data on nonhandicapped students appear to be very similar as to wages and benefits. Youth in America, in general, live in poverty, and have very poor health care.

There are examples of special education graduates doing well after high school, but most of these students are receiving some type of ongoing support services (Wehman, Hill, Goodall, Cleveland, Brooke, & Pentecost, 1982).

A large percentage of mildly handicapped students never graduate from high school - they drop out. Data on dropouts from special education are difficult to determine but there is considerable evidence that many mildly handicapped students do not complete high school programs. Zigmond & Thornton (1985) report a dropout rate of 50% for LD students, while Hasazi, Gordon, & Hull (1985) report a rate of 35% for all special education school leavers.

A final point is that of comparison to some norm or standard. Even after collecting all these data, to what standard do we compare our results? Certainly using a nonhandicapped cohort provides some comparison. But what about the iniquities that many of our nonhandicapped youth experience? If we can report that youth who pass through the special education system are no worse off than youth who have not been in special education, can we be satisfied? I think not. We must develop some concrete standard by which to measure our outcomes.

Overall, I believe the data do not support the notion that "things are going well for special education graduates."

Rules of Evidence

I propose the following notions as guidelines for conducting future effectiveness research. Four points seem to be relevant: 1) operationalizing outcomes; 2) analyzing data by subgroups within the total special education population; 3) measuring the process variables (the independent measure); 4) attention to the size of Ns and national as well as local representation.

Outcome measures. Obviously, I question many of our current outcome measures. However, at a minimum, we should consider the following: skill levels (e.g., achievement test scores); success in postsecondary training (e.g., graduation from college or vocational school); job acquisition (salary level, benefits, promotions); friendships; reported satisfaction with life; and absence of negatives (e.g., legal problems, institutionalization, being unengaged). Our current data base consists almost exclusively of information on jobs, salary levels, and postsecondary education. We must expand these measures to include the more qualitative aspects of life.

Subgroup analysis. The special education population is incredibly heterogeneous. I strongly recommend careful analysis in terms of the following subgroups:

1) Type and severity of disability. Even with the controversy concerning inappropriate labeling, we cannot squash together various types of disabilities. We must give careful attention to levels of severity, especially in regard to mental retardation. As obnoxious as IQ scores may be, indiscriminate groups of "MR" are even more annoying. Outcome studies need to identify the populations as finitely as possible and report data in terms of subgroups. Data can always be aggregated; seldom can they be divided.

2) *Gender*. There are considerable data to support the view that gender influences outcomes. Within each disability category, researchers should consider gender as an important factor.

3) *Ethnicity*. Regardless of the reasons, ethnic minorities are overrepresented in the special education population. We must analyze our results by ethnicity so we do not miss possible trends. Have no doubts, if we do this, we are *all* going to feel uncomfortable.

4) *Social class*. There is reason to believe outcomes are directly affected by the level of the family social status of our students. Students from underclass families do less well than students from the middle class. Even though this marker is most difficult to obtain, we must begin to analyze our results by social class. I fear that social status accounts for the major amount of all variance in our results.

Measuring the process variables. Simply collecting data on the postschool status of special education students is not sufficient. We must begin to obtain follow-along data on the types of programs students receive while in school and correlate the programs to outcomes. The inpact of integrated versus segregated programs on student outcomes is one type of issue to address. In addition, we need to follow students who have received systematic vocational training as compared to those who receive only academic training. The issue here is that of programs which offer sequential vocational experience, not simply attendance in a "world of work course." Data on attendance *and* skill acquisition while in the secondary program are needed. The only way to do this is to implement systematic data collection procedures while students are still in high school, and follow these students throughout their high school careers and into the adult world for several years. This would truly be a follow-along study.

Population considerations. As researchers conduct their studies, they must take care to collect data using large samples that represent various geographical regions. Urban, suburban, and rural areas need to be sampled as well as students from industrial and agricultural communities.

Many current studies have trouble locating significant numbers of special education school leavers (usually about 60% are contacted). The most mobile students tend to be missed. My guess is that the students who are difficult to locate probably are experiencing less success than those students who are discovered with the exception of the Iowa study (Sitlington, 1987) the "hit rate" of most studies is suspect.

Summary

Education is the major entitlement for all citizens of the United States. Our educational system provides the process by which our youth enter the adult world, prepared to be happy, productive citizens. Many students served in special education enter the adult world with a minimal likelihood of achieving a successful life. These students come to us not of their own free will but rather by default, when other options fail them. We must carefully celebrate our successes, and freely admit our shortcomings. This problem demands bold, persistent experimentation. It is common sense to take a method and try it; if it fails, admit it frankly and try another (I paraphrase a quote by Franklin D. Roosevelt on announcing the New Deal, which is cited in Boorstin, 1987, p. 84). To do otherwise is to let down those who place their confidence in us. We cannot be frightened off by the rapids in our path.

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View From OSERS

Martin J. Kaufman, Director
Division of Innovation and Development
Office of Special Education Programs

In many ways, I'd rather just try to field questions, but there are a few things that people have asked that I address directly. So, I'd like to take half of the time to try to communicate what the Office is doing about some of the things people have expressed concerns about all year long, and then open the session for questions to see if we can share with you what may not be apparent, oftentimes, and you can help us, quite honestly, in our thinking. Often, when you have concerns about how the program can be improved, we've probably got even more concerns. Often our problem is that we are not quite sure how to solve that which we know is wrong. And these conversations, sometimes, can give us the confidence and direction to move. Often we have what I think of as "motion stubbornness." In the absence of any confidence, it takes quite a bit of force to move us, because sometimes the evil we know is less scary than the one we don't know, so that one becomes concerned about how much to refine the system, change the system, etc. I hope that our conversation will give us some direction in addressing some of the issues that I know continue to concern everybody.

Let me start in the context of the review system for a few minutes. Last year Tom and I said that we were going to examine the review process. Some of you, I am sure, had copies of some thinking that Tom put out on the review process. Internally, that material and input that we've received is being massaged into, "What actions could we take?" Let me talk right now only

11[illegible]

for the research program, because in many ways, our ability to implement suggestions and the form that they should take is somewhat influenced by the particular programs. For example, DPP, because of its volume, nature, and purpose, faces a somewhat different situation than that which confronts us in the research program.

Last year, thanks to enormous effort by Doris Cargile to get out and update our pool of field readers, we now have a list of about 500 names in the file. One might say, "Boy, that's a pretty large database." Let me tell you, in this last year, I used every one of those names in trying to come up with the panels. That is, there were people who weren't available on the dates we asked about at the beginning of the year. Sometimes people weren't available for the dates, and we had plenty of people in one instance and not enough in another instance. In some areas, because of the number of people who make application to a particular competition, we exhaust rapidly the names available to us. That pool is none too big.

On the other hand, the department has not been able to grapple with the issue of the criteria for determining whether or not someone is an expert. That is, we will look at the vita for some correspondence to the areas of expertise checked as opposed to areas of interest, and make some cursory level of judgment that is not a terribly discriminating one. It's hard to look and say, "Well, how many databased publications should make somebody an expert, as opposed to opinion articles that are published?" How many publications altogether, if that's the criterion? That process is one where the names in the pool are self-identified, and there is, at best, a cursory screening that occurs. Normally, if there is an issue where we can't make a judgment with the first screening, rather than discarding the vita, we send it back to the individual and say, "We're unable to make this connection. Could you provide us additional information?"

The proposal that has been put in front of us is to create registers, as you know. Those registers, in the area of research, will not be very limiting, because, in fact, we need that many reviewers. On the other hand, it gives us an opportunity to develop a process for creating those registers of reviewers, and our intent is to work with you in setting the criterion process by which the various registers would be created. But given the numbers of applications that we receive, and the number of people submitting, it's not going to be the equivalent of a standing panel. The reason we are using the term "register" is because we need multiple panels. We can't get around the problem of the volume of applications that need to be reviewed, which creates multiple panels. All of those involved in the paneling would be considered the register. That will be something which we will try to operationalize during the coming year. It is unclear, at this point, whether we can get it in place even to pilot it. However, our hope is to be able to pilot it during the coming fiscal year on some competitions, at least, if we can get far enough along and are comfortable with that system.

The criteria: Up until now we've had Edgar (?) regulations. I know many of you have been in to review, as well as, obviously, writing applications. When you're writing the research application, you look at some of the criteria, such as evaluation, and you say, "What do those five points really mean, and what do they want?" We now have, as a result of some changes in our overall departmental regulations, the ability to remove some of the criteria, such as evaluation. And we'll be doing that. Again, we cannot do that this year without affecting all of the schedules for this coming fiscal year, but we will do it during this year so that they are in place for FY90 applications. And those changed evaluation criteria will be out there for comment.

One of the things we've heard, and have tried to correct, and seem to have overcorrected, is the number of points that we allocate to different types of criteria. For example, it used to be, if you remember, that we had 30 points on technical soundness. And then we increased it to 40 points. Well, the reason that happened was that we were beginning to see projects being funded that were seriously technically flawed, but that had good ideas. Well, now what's happening, when we look at the process, is that we get projects that are technically sound, or even elegant, in my own thinking, but that are not looking, necessarily, at a very important problem. And we're not sure what to do. I'm not sure that 35 points is the answer--to split the difference. It would be nice and simple. I just don't have any comfort that that's going to fix the issue.

We've looked at two-stage review models, such as NSF, which starts with a pre-application review and then an invitation to an NID to those that make that first screen. Our type of model has two stages, where there is a review of the full applications, which determines their technical merit, and then a second review, which then works only with those that are technically meritorious. And that has its problems. But, we've been examining all the various systems, and we are trying to pick, we hope, the best of those choices. While we've had some communication with you, I wanted you to know that the issue has not died yet. We are seriously and cautiously working with it, so that, we hope, our solutions don't become worse than the problem.

Another aspect of the review system that we are increasingly aware of, is the difficulty of communicating our intentions with some of the terms we use. We sometimes use the word "model," sometimes we use the words "model development," and sometimes we even talk of "model testing." What happens as a result is that there is confusion on the part of the applicant, and confusion on the part of the reviewers as to what the expectation was. It would be better if we just said, "We're looking for you to develop a model." Sometimes we'll say "test a model"; sometimes we just use the word "model." What is it we expected, and what is it you wrote? What that does is lead to too much randomness in the review process. But the danger of correcting that problem is that the more we spell out, the more prescriptive we potentially get. And that wasn't our goal, either, to get overly prescriptive. On the other hand, we recognize that we are going to have to expand our discussions, at times, because there's no common lexicon to communicate with. Let me give you an example. If we say "model development," what happens if you already have a model and you want to test it? Does that mean you're not eligible, that you're non-responsive? That's an issue that came up. Somebody already had a model, and what they wanted to do was actually test it, but it was developed--they weren't proposing to develop the model, they were proposing to test the model. And we said we wanted to develop models. What did that mean? That's where the confusion can come up. Let me give you another example. We say "develop a model," and an applicant puts the whole package together and plans to test it for its effect. And the question is whether we wanted them to test the components of the model so that as they constructed the model there was some amount of evidence as to what was contributing to whatever effects occurred. And does a project that does that get more credit, or points, on technical soundness than one that just looks at the model in its entirety? We also talk about replicability. In some of our model programs, replicability means, "Can you get others to replicate it?" When we use the word replicability, what we want to know is research about its implementability in other sites, under other conditions, its replicability with other populations; that is, the generalizability of those effects that you have found, not just whether you can get other people to pick up the model and do it. But we don't say that either; we just use the word "replicability." Does it mean we ought to be studying the issues of implementation? Should we be studying the independent variables, and is there an expectation that during the model development the implementability of the model and those other factors will be evaluated? Consequently, it becomes difficult to get you (the applicants) a consistent review, because we are leaving it up to you to decide what to put into the application. We're also leaving it up to the reviewers what to comment on. Our inclination is in the next set of priorities to try to spell that out more clearly. It is imperative that you comment on those priorities. There is no magic in writing the priorities. What they represent is our collective best effort to listen to what you've been saying, to look at what happens with the competitions, and to try to bring all that together in the way we write the priorities. They used to be about four, five or six lines; they stretched to a paragraph; some in the next group of priorities are going to to be three and four pages long, as we try to communicate the intent better. We are not trying to be prescriptive; we are trying to give you more background information. It used to be that we could put it in your application package as "background information." This year the Division of Educational Services sent out that consolidated application package where everything is listed. Well, they are going to do that with the notice and with the application package. Ideally, with the notice you could see everything that we're going to announce, as the Office of Special Education Programs, in one place, and see it for the year. We're struggling with that, and I can tell you as of Friday last week, we're going to have to break the package apart a little. Otherwise, such areas as the field-initiated research won't make it in the first half of the year. In order to get field-initiated out and anything else we want to do in the first quarter, meaning October to December, we have to get it out in the Federal Register by August. We also have to go through a clearance process within the department, a clearance process within the Office of Management and Budget, and then there's a 30-, 60- or 90-day comment period, depending on the programs we're operating with, that we have to allow for comment. One of the criticisms of the program is that we've been putting out proposed priorities at the same time as we have closing notices out. Then when you make a comment you usually have the response "no change made," because if we change it, we've got to do something about the competition we just ran. Obviously, this doesn't allow for much public comment. We made a commitment to try not to run a competition without allowing for final publication of the priorities, not just the proposed with a closing note. That extends the timelines to permit feedback. And it should be that way. This year, one more time, we're going to violate that commitment on field-initiated, on the assumption that we won't get a comment back that says, "Don't run it." And probably if we got that comment, we'd say, "No change," unless somebody had a new reason for not running it that we hadn't heard before. And that will let us get it out roughly on the same time schedule you've begun to expect, which is sometime in October, if it all goes well. The remainder of the priorities will be out in the fall, but we'll try to get them out as a total announcement. Now, as a notice, that's fine. What you're going to see is a difference. You're used to seeing our blue application packages in DID. The application packages are going to all be consolidated into one massive Federal Register that will be different from the notice. In doing that, we lost the ability to put in background statements. Those background statements have been reworded so that they're part of the priorities statement now. Before, we had a background and a priority statement, but we've had to put those together because of departmental formatting issues. I have some concerns about it. The best thing we can do this year is get comments from you after you've had a chance to try to deal with this format, and that will help us within the departmental review process, as to whether this so-called "efficiency" is the way to go to make it clear to you how to prepare an application.

With regard to proposed priorities, if I get ten comments, I feel like I've been overwhelmed by comments. Typically we get comments from the associations and one or two other people. Some of the people in the field don't connect the priorities with what the competitions are going to be. The priorities have a direct relationship; they are going to be the competitions if you don't make comment. The correspondence is one to one. That's not just an activity, that is what the research agenda will be, those priorities. The problem is people see them as proposed priorities and don't connect them with the applications for the competitions. The process is in place, if we use it, for you to help us to look at the content and the substance of particular priorities and whether we should have been asking about other priorities. That is the way to influence that process.

My personal approach is that I start with the concept that what we're dealing with is a marketplace where there are producers, and exchangers, and utilizers of ideation, and that what we're trying to do, in my opinion, in the program, is not to distort the natural marketplace of ideation, but to provide resources so that it is a vibrant, workable marketplace, without the government distorting that marketplace by its support. The priorities, hopefully, over a few years, deal with the breadth of ideation that you all, and others, represent. In listening to ourselves, you get a sense of being overwhelmed, at times, by the breadth of interest and diversity that we represent. And it seems to me that the program should permit everybody to find a place over a two- to three-year period, given the resources that are available through Part E.

What I'd like to do now is to take a few minutes and try to relate some of the priorities to some goals. We've tried at various times, like most of you, to put together goals and objectives statements, and I keep going back and changing them and refining them, because, at times they don't provide the direction that I'm looking for. The concept of a marketplace, when superimposed on these goals and objectives, tries to give some direction to the intent, so that our goal is to contribute to the ongoing advancement in knowledge and practice through the research process. The process is designed to increase understanding and encourage application of information which, when transferred into practice, will improve the education of infants, toddlers, children and youth with handicaps. This is not to suggest that this is the only way practice will improve. It is to suggest that that's what this program is about--using the research process to do that, while recognizing that there are factors that will also affect practice. But this program is designed to support the research process.

The budget for doing this is something I want to talk about in pretty frank terms. One of our jobs, as administrators of the research program, is trying to plan and manage the resources available to you as a community. In order to do that, you need to have a continuing sense of what we're trying to manage and fit into the various competing priorities, needs, desires, etc. If you want to look across ~~the~~ new monies, you'll notice, in 1987 we funded 70 projects at 9.3 million dollars. In FY88, we funded 57 projects at 4.9 million. Part of that is because we've got a large increase in the research budget in that year, and because we can't spend the money in out-years (we have to spend it in the fiscal year it's appropriated), that's what that bubble looks like. It was created by change in our appropriation level that year. In FY89, you'll notice, we have 54 projects projected at 4.38 million, and in FY90 we have 38 new projects budgeted at 4.7 million. What's happening is that projects are becoming more expensive and lasting longer periods of time, and that affects the number of awards. And that's a dilemma--how to keep juggling the nature of the questions you, as a community, raise. It also affects the issue of the number of people we can support, given the budget level. And if you notice the continuations, which is the second line of that table, in 1987 we had 8.66 million, in 88 we have 12.3 million, we have 12.86 million, we have 12.5 million. The amount of money is holding about constant, in other words; we have about 25 percent of the budget. Of a 17 million dollar budget, we've got about 4.7 million in new monies, so we're just about at that 25 percent rollover. You will notice that the total number of projects has decreased over that four-year span. The reauthorization hearings for these programs will be this fall. That is the time to let people know what your needs are. We are competing in the research community with other priorities--training personnel preparation and all of the other discretionary programs. But as a community, you need to know what your resources look like, at least as they relate to this one authority.

One of the other issues I want to discuss is out-year budgets--continuation proposals. I'm actually budgeted somewhere out to 1991, at this point. My projections for what I can do, and what we as an Office can do, in the way of offering support and new programs, is contingent upon some assumptions about the out-year budgets. If you come in and give us a flat budget for four years, I have to wonder, "Now, are they really not going to come back and ask for more money." At the same time, somebody else is showing a ten percent inflationary progression, which means that you can have projects that start at $200,000 ending at $300,000. It gets out of hand because I can't plan for the new priorities. In the past, we have not had to be very arbitrary. At this point we're running 23 competitions. That is, with our continuations and new projects, we're trying to manage 23 different priorities. And it is essential that we control those out-year budgets. So sometimes we're getting on the phone at this point, because I don't know where to cut the budget. I could arbitrarily tell our project officers, "Go in and cut it somewhere." But I'll say to you, "This is what's available. You go cut it"--because if we don't cut it, we may have 25 projects instead of 38 projects. In the past, we've had more latitude to deal with these out-year budgets than we currently have, because we have so many of them to try to manage. If we were off on 78 projects, what we ought to do is say, "How much are we off?" and multiply it by 78, and then subtract that from the new projects. Everytime somebody asks us, "Can we have a little bit more?", what we're trying to do is to juggle your need against what that's doing to the new competitions. That's the source of the pressure that you get from us at times; we're trying to figure out the appropriate balance between the news and the continuations. And, because the continuations may come before or after the news, it's not an orderly process. They're scattered. Therefore, we have to be careful, because otherwise the last person in, not for any other reason than they were last in, will run out of money. At the same time, we're trying to zero those budgets out so that we don't send any money back to the treasury at the end of the year. That's why you sometimes see us put you on hold. That's the "fudge factor" at the end of the year--to see if we can even it up. That is the background of what at times you see as negotiation issues; if you have a feeling that there is something going on, that's what is going on. In my opinion, this is also something to be concerned about as a community: How do we balance the nature of the questions we have with the need to support a large community, in a way in which the probabilities of finding support are reasonable? We feel the pressure of that, of not knowing how to deal with the questions you're asking in light of the resources available and the probabilities of finding funding.

The first objective we have is to increase understanding through the production of knowledge by stimulating and supporting research activities to improve the education of infants, toddlers, children and youth with handicaps. The priorities for 1989 and 1990 are in the midst of finishing departmental clearance and have not made it to OMB yet. We hope that, within the next month and a half, we will complete the clearance process so that we can get these proposed regulations out to you. But that means that today I cannot spell out those priorities in detail. I will, on the other hand, talk about some of the areas that I think will align themselves with those priorities.

There will be a field-initiated research program. There will be in 1989 a student-initiated research program. We are proposing to drop it in 1990 and to put in its place an initial career award program for people in the first three years upon receiving their doctorate. And we are asking for comment about dropping that student-initiated research program. I've mentioned it a number of times at conferences, and I've mentioned it here. We've had objectives for that program, and the response we get to the program doesn't seem to align with our goals. And in light of other feedback that we have received, we think that an initial career award might better meet some of our objectives. But we are asking for comment on whether the program should be dropped. If we do not hear from people, it will be dropped.

We are going to institute, we think, some type of program that tries to deal with a range of problems that we're not happy about, nor are you, from what I can gather. We talked earlier about the issue of the 30 points and the 40 points and the types of projects that seem to be getting approved and disapproved. For example, there may be an applicant who has new methodologies, or a content that's new where the methodology is not worked out, or new questions. There may be someone who is foraging on the edge of a forest, and it's hard to be elegant when you're just beating away at the trees and brush to get in--there's no highway or road in to that inquiry. Those proposals don't survive. The comments, oftentimes, are that they're good ideas, and why doesn't the investigator go back and work on it some more? But that's why they're asking for the money, so that they can work on it some more. Our thought is to try to structure a program where we could deal with some form of what I will call pilot studies. This program would give these people an opportunity to work with some support.

Another problem area is that we get applicants who want to do a synthesis. If somebody wants to do a synthesis, and that is competed against somebody else's three-year or four-year research project, it doesn't survive very well. They're asking for maybe for $35,000, $40,000, $50,000, and it gets killed against some elegant $350,000 four-year project. That's the kind of thing we're trying to find a place for. We have applicants who want to look at extant databases. They have the data they can get their hands on, but they're looking for some support to analyze it, to add to it a little bit. Those applications don't do very well. There are some people who have come in wanting to do some sort of utilization study on the same scale, and they don't tend to do very well either. We're trying to find some vehicle, some umbrella, that's both of different duration and with a different amount of funding than field-initiated, but that is basically a derivative of field-initiated that would let people take that divergent set of ideas and find some support. That will be in the proposed priorities for 1989.

We have looked at a range of administrative instructional issues: How to teach children? How to organize to teach children? We've looked at service delivery issues, but what we've not looked at, in recent years, is what we're teaching, including some of the implications of the scope, the sequence, the complexity, the information density as they relate to the learning characteristics and needs of handicapped children. We will propose some areas that deal with examining and analyzing some of the implications of not just how to teach children, but what we're teaching.

I want to make sure that we understand that special education and educating handicapped children are two different things--we can educate handicapped children in any setting, regular or special; anybody could be delivering that service. That's not true when we talk about special education--then, depending on the state, only certain people can deliver it. There are certain definitions of what is special education and what is not special education, distinct from the education of handicapped children. I want you to know that on our applications we're trying to communicate that distinction between the two terms. When we talk about educating handicapped children, that could be in regular or special education. If we talk about special education, we're talking about that part of the child's educational experiences that meets the definition of special education.

One of the concepts we are addressing is that teachers, in educating handicapped children, are meant to be able to individualize and adapt instruction. A number of the questions we've had really seem to be questions of contextual fit; that is, how do our expectations fit with the realities of the context within which we want people to perform. We talk about teacher planning. Are we talking about unit planning? Are we talking about lesson planning? Are we talking about individual planning for a child, as we start asking teachers to do individualized instruction? Where do they plan for that? Is it done at the unit level? Is it done at the lesson level? Is it done on an individual-child-basis daily? When, in fact, do we expect that to happen, and realistically, how many times and with how many kids can that happen? And what seems to foster it, and what seems to impede it? Similarly, with adaptation, how is that going to happen? We are going to see an area of research which tries to get at some of these thick questions concerning teachers and the types of things we want them to do that we think will improve the education of handicapped children. We are trying to provide some opportunity for inquiry and intervention work in that area.

Another area is one that we have been addressing through an institute, but that we will open up now to more single investigative inquiry. That is the area of non-dominant language studies. We recognize that the school has its culture, the children have their culture, and the families have their culture. They also have their languages. In some instances there's a cultural incompatibility, and in other instances there's a language incompatibility. For these non-dominant language groups, we are trying to provide some resources to permit further investigation into ways that the language and cultural compatibilities can be made as consonant as possible.

Another area that we are supporting started off as an SED, or behavioral disorder (depending on your preference), line of inquiry. Over the last two years, as many of you are aware, we ran competitions concerning seriously emotionally disturbed children. At this point we feel that problems of getting seriously emotionally disturbed children into their neighborhood and community schools is probably more a problem of figuring out how to get mental health services and school services connected. Schools will tell you, "Treatment is not our responsibility." We have a hard time connecting with the mental health services. The actual educational interventions that we've seen in the last two years, quite honestly, don't look terribly different than Project Re-add (?). The educational part of it isn't changing that much, but that's not what's causing the children to be placed out of school into separate facilities, etc. The question really is how to get some better linkage between education and treatment, as opposed to just stabilization and maintenance of behavior for the children. Consequently, research didn't seem to be the next step. The problem seemed to be requiring some form of systems change. Instead we moved the problem to one that was a little broader. While SED children drop out, or leave school, at a rate higher than any other category, there are other children, clearly, who are leaving school at nearly the same rates. The area we are looking at is moving to an "at-risk" (of leaving school) category and changing the constructs we are looking at to try to identify the problems. For example, if it's predicted that kids are going to drop out because they get poor grades, the answer isn't really to give them good grades; that's just dealing with the symptom. We're going to try to move you to a construct that we're going to label engagement; that is, how do you get children, in a broader sense, engaged in school (tape turned over).

We're going to try to do something that we've been getting input on, but when it comes to us, people are asking for assessment areas. Typically what we see is a proposal. Somebody either wants to develop a test or norm a test, without, always, a very compelling reason as to why we need one more test, at least from a knowledge advancement point of view.

In the area of technology, we're going to try to provide an opportunity for using technology as a vehicle for either advancing the types of constructs we're measuring, the items stimulus that we're using, the response categories, or the scoring criteria. The idea is to use that not to develop tests, but to advance assessment practice. We're trying to use the technology as a vehicle to break what seems to be a static mode, possibly constrained by paper and pencil realities. Possibly the use of the multi-media available in technology advances will force us to think a little further ahead as to how we might press the issues of assessment.

These are the areas that you will probably see in some form or another as the proposed priorities that come from the areas of interest that we've been able to identify. The priorities that are going to be announced will cover a two-year period. They cover 1989 and 1990, because by law we have to publish the priorities for the research program two years at a time. Again, what I would encourage you to do is look at them closely and please communicate your thoughts about them, or about ones that you feel we've missed altogether. What I'd like to do now is open it up and let you ask questions, having given you that general information to work from. Naomi.

Q. I hope this doesn't seem impertinent because SEP has been very good to me, especially this year. I'm curious about how these research priorities, this litany of research priorities, gets established? Who comes up with these ideas? Some of them are wonderful. Because I know that in my research program, my next good idea comes out of my last good idea. And I'm wondering why, or what proportion of the research money is in directed priorities? And why it isn't in all of them, in field-initiated, so that researchers can pursue a line of research without having to change, or at least change our papers, to get re-focused to meet new directed priorities year after year after year?

A. Let me see if I can break that up. First of all, I think it's important to say it's real nice when people come up to me and say thank you for a grant, but if I accepted that thank you, I would have ten people who didn't get funded looking for my neck. And since I really don't want that ten looking for my neck, I can't say that I had nothing to do with those, but that I did have something to do with someone else getting funded. There is a peer review system, and it's your colleagues who, year in and year out, competition after competition, make the selection process. After they leave, it would take a real error before the Office of Special Education Programs would override the rank order derived from the peer reviews and recommendations. It is not a matter of judgment; it is not one of preference. The only opportunity we have to influence that award system is if an error is made, and then it's going to have to be a whopping one for us to interfere. So, while I'd like to thank Gene and Naomi for their graciousness, it really is a peer review system. That's why it seems important for our office to keep a communication going. For the system to work, it takes 500 people, in some level of synchronization, to make it work, or it's random.

To go to the priorities, let me assure you that I read a summary of every new project going out, and I read what was accomplished and what was proposed on every continuation or summary. The staff reads that stuff in detail. Typically, we have communication, at some point or another, with many of you. Let me assure you that your findings and your discussions with us seriously influence our own thinking, in trying to say, "How do we incorporate that?" The question is not one of exclusion; our attempt is to keep trying to put the program together so that it permits the maximum amount of inclusion of the community with the wide range of methodology and ideation that are out there. What happens is the field gives us some ideas, your research produces some, clearly, and the administration produces some. If we get comments, which we have not been getting, they do influence the process. Remember, they are proposed, they are not final. The attempt is for us to aggregate and cluster them in a way that we think, over a two-year period, will permit the pluralism that's in the community to find a home. I can tell you that while some priorities are going to hit some people, they miss others. Not every priority will please everybody. They are not designed to; that's not who we are as a community, nor should we become that narrow in focus. What's not happening is we don't get good feedback from the research community to the proposed priorities. People aren't aware of what the process is.

Q. Along that line, if we think back to yesterday's lunch, we have to be sensitive about how we get funds, and you mentioned some things like the administration's position and input into priorities. Certainly there are some things where maybe it is to our advantage to support, or not negatively comment, on these priorities as they're listed. And if you could help us with that, maybe we could help you. But most of the people that I've talked with would rather not see a list of priorities this year and another list of priorities next year or two years later. We tend to feel that good research would find its home in field-initiated competitions, maybe even have two or three field-initiated competitions. Maybe one could say, within each of those two or three field-initiated competitions, that certain things are of interest, but not have a binding kind of thing where, if you don't fit into categories one through seven, you're then in the small pot called field-initiated. So if you could give us some guidance, we could do more than just write in and say, "Please don't do priorities, just do field-initiated research."

A. Let me answer Naomi's question, which was, what percentage of the budget is in field-initiated? In any given year, I think, off the top of my head, it's around 6.5 million. Anywhere between 6.5 to 7 million. It goes up and down depending on any given year. It'll probably creep up over 7 million at the moment. So, you're running around 40-45 percent of the budget in ~~that~~ field-initited category. Some of that money is also being used for institutes, out of that 17.2 million, so that some of you have asked for five-year, large, multiple-investigator inquiries. Some of the money is used for institutes, some of the money is used for field-initiated, and some of the field-initiated are five years; but they are not necessarily programmatic or multiple investigator inquiry. They tend to be different varieties. So there's also five-year support, but for other than institute support, we have the directed research priorities. One of the things we have been striving for is to get those into some streams. I have a copy here of something we put together on our competitions. Maybe I can get Kathleen to send you a copy. Let me give credit for it to Ed Kameenui. Ed tried to bring some ordering to a number of competitions we've run over the last several years, starting with Enhancing Instructional Program Options in 1985, which looked at the range of pre-referral program options for providing instructional and evaluative services in regular education. In 1986, we ran a second directed research program called Increasing Teaching and

Learning Efficiency. These projects were designed to build upon the teacher and school effectiveness research literature, by looking at what amount of the variance (if we put in effective teaching, effective school practices) could be accounted for in educating handicapped children. In 1987 we ran a competition called Educating Learning Disabled and Mildly Handicapped Students in General Ed Classrooms, and there we tried to look at how you could deliver special ed services in regular ed classrooms. So you had this progression; it was all in a stream, but it moved through a set of variables. We then came out in 1988 with the School Building Models competition, which was an attempt to take all of that, put it together into a more comprehensive package and say, "Could you take the individual parts?" and "Would they sum into a whole?" That was this year's competition. So there's a progression in trying to advance the inquiry. In each year there were seven, eight, nine projects, or whatever, funded in that progression of studies, where the knowledge was advancing down that stream. What we've been trying to do (that's probably our best example to date), is to make predictable what those streams are, so that while the particular priority may change, the change is just advancing that line of inquiry rather than letting it become stagnant. We try to move that part of our "portfolio" ahead in a more cumulative fashion than might happen otherwise, in trying to balance the directed competitions with the field-initiated competitions.

To answer Lyle's question, the politics, in my opinion, of the priorities are that the program belongs to the community. That program is authorized and appropriated to conduct research, and you, as a performing community, are given a number of opportunities to influence the priorities. The only politics is in expressing your viewpoint. You need to say what your feelings are, and that program ought to be responsive to the community. But if the community is quiet, then what you are left with is that we try to make the best professional judgements we can, internally, with the information available to us, given the various forces I've described. There's nothing mystical, I want to assure you, behind that. And there's nothing I won't talk about, if you want to ask where they're coming from. I don't know if that gets at it enough.

Q. Can I ask you a question about the student-initiated you just mentioned? Is it possible to have a competition in which you could open it for new investigators and for which many students (a few words inaudible) could still apply? Is it possible to have both, rather that one versus the other?

A. Monetarily, the answer is probably "yes." There's some question as to the return on investment, which might be worth talking about a little. The other reality that's happening is that I have, as of next week, a Branch Chief and one staff person in the directed research branch. There are only so many competitions that we can run, given our staffing. Part of our problem is that the process is extending us to the limits of our resources, in trying not to make review errors, as well as not sending out poor priorities at the front end of the system. I will tell you it is the first time in 18 years in government that we've begun to look at our priorities, not only according to what the field needs, but also our ability just to get the competitions developed, announced, reviewed, and back out again. The realities are, if you comment, we'll see what we can do with it, if it seems to be the voice of the community.

Q. Can I follow-up on that? What if about 15 or 20 people commented when the priorities came out, suggesting that you not have specific competitions for these priorities, but you have two competitions, or three competitions in the year in which you have certainly made known, through the literature, some of the areas that are of concern, but not as a specific competition? It seems like you would allow the freedom for any strand to develop, if there is good science to develop it. It seems like you would cut your costs of competition management significantly, if only by reducing the number of different things that have to be put into the Federal Register. There are also a lot of other cost savings you would have there. It seems like there might be some value all the way around, and it might promote more creative science.

A. We would take a good hard look at that, Lyle. What that is called, by the way, is an invitational priority, which we do have sometimes. That means you're not given any particular points for responding; those are just the areas of particular interest that we're trying to encourage. Sometimes we do go with those.

Q. Two questions. What about research on families of handicapped children?

A. In the example I was talking about where we are trying to move the construct to engagement, I am assuming that the family is a critical piece of achieving engagement--it's not the only one--but I'm assuming some people could find a home for families there. I think the issue of non-dominant languages has the potential for including work on families.

Q. O.K. The second question is, would I be considered as a reviewer if somebody submitted a project from North Carolina?

A. Under the registry system, that issue has to be dealt with. Our request was for the answer to be yes, and what we would have done is had you on a panel other that one where there was a North Carolina application. They let us do that only in the field-initiated research competition, and considered you in conflict of interest in the other competitions this year. The year before we ran all the competitions under your being eligible to be a reviewer. This year they changed their minds on us, and it has been a back-and-forth interpretation, over the last several years. We're hoping that this registry issue will cause them to swing back to saying, "No, you would be an eligible reviewer."

Q. It's hard to understand why NIH is able to do this all the time, and you're not.

A. Well, it's individuals. It's interpretations. You're not dealing with something I can put my finger on, at least as I've looked at it, and be able to nail it. You go to where the source of the decisions are and it's their interpretations.

Q. When you want feedback on your priorities, my question is, what structures do you provide to have that feedback and how broad-based are those structures?

A. At this point, there's no structure at all. It says "write us a letter," and they give you a name and an address.

Q. I think that if you really want the feedback, you're going to have to broaden the base and create the structures for the feedback to come in, because you give us the priorities, and this is the only opportunity we have. In terms of us participating in setting the priorities and getting direct feedback on what priorities come out, I think more structure needs to be there.

A. I'd like to go back to Naomi's question and the theme that was addressed in a couple of the others, and that's the whole issue of the presence of directed programs versus the field-initiated, or open checkbook. For those of us who work on the other side of it with the senator's office and those sorts of things, I don't see how you can run this system unless you have directed programs. I might debate with you the political influence--I'm not sure it is totally apolitical, but I think you can't establish a case for funding if you say, "We're going to tell you what we're going to work on. We're going to leave it up to field-initiated processes, where directions are picked by individual researchers and programmatic lines of researches are going to be conducted. And we're going to assure you a quality control process but no directives." I can't see any of us building support for funding through the processes that exist now, unless there is some crystallization of direction at the front end, and not just a process of recording accomplishments at the other end. A few years ago we went through some pretty rough times trying to protect this funding, and I think the research community, as far as I was concerned, was the most naive and least responsible and least present when we were fighting for those dollars. Service people were out there, training people were to some extent, but the research community seemed to be very unaware of what could be done and what had to be done at that particular level. If you work at that level, with the congressional staff, and you see what they have to deal with on a day-to-day basis, I don't think they could do their job without some crystallization of direction. You're not going to get the dollars, I don't think, unless you're prepared to do some front-end directing. Where that might come from, is another issue.

Q. Would it be possible that these meetings might enlarge the project directors (rest of question inaudible).

A. Certainly the "have-nots" would claim that, let me assure you of that. I've seen a couple of things happen. I know that Sid Miller of the Division of Research at CEC has talked to me about trying to figure out a mechanism for bringing up suggestions. I saw something that National Inquiry tried to put together as an initial set of research agenda issues that I looked at. I don't know that there is a single vehicle that you'd want for that, but I do think that some systematic ways of inputting--in order to have opportunities for yourselves, they have to be pretty robust priorities to allow enough people to compete. You might be interested in knowing, actually, that on most of the priorities, we're ending up in the neighborhood of 40-50 applications to a directed research priority. We're getting about 165 or so to field-initiated. I'm trying to remember about the student-initiated; if one of the staff is here that remembers they could share it with me. Does someone remember the number we had this year? About 40, Bea says. This is just to give you a sense about what the volume is, what the response is. But I do think the community needs some opportunity to hash it out, to try to think about it. What happens, and what you don't want it to deteriorate into, in my opinion, is just individual interests, being self-serving. And you have to get it so that either everybody sends in their individual pieces and we try to separate them up into some streams--that might be a good way to go at it. But I'm certainly open to any suggestions in trying to facilitate exchanges that would let that happen to all communities.

Q. When the panel meets to review a directed research competition, that seems to be the best opportunity to get feedback--it's after the fact, once it's already out of the barn. But my recollection is, after having read those applications, some real good feedback (rest of question inaudible)

A. And that is part of the input we get. That's one more example of where we're getting it--from the panels after they've looked at them. It ends up being difficult at our end for reasons that you're not even aware: When we call and ask people to be panelists, it's supposed to be a confidential system. What happens is you mention, "I'm coming to Washington" to somebody. The next thing I know there are all sorts of phone calls going on. It's not in your best interest to not keep it confidential when you've been called. Quite honestly, there is less opportunity for the system to get convoluted if, when you get a call to review, it's treated as confidential. Otherwise they are not blind reviews, and the system has the potential, at least, for some abuses. And it is in your interest to operate that way.

Q. We were just talking back here whether that really is the case because, again, to mention NIH, everybody knows the panels there, and it's public information when the meetings are held.

A. There are pros and cons to what I'm going to say. We guarantee three independent reviews, and we share those with you, for better or worse. All you have to do is look at the variability in the reviews you get back, and you know you've got three independent reviews. Now remember, that's not what they're offering you, and there are pros and cons to that. I'm not suggesting that ours is foolproof--it's just a different system. Consequently, the confidentiality operates a little differently.

If we have no other questions, I would like to once again thank everybody and ask you to fill in those evaluation forms. Your ability to help us figure out how to make this a rewarding day and a half and worth your time really is critical--I think the people who are on the planning committee will tell you we really did use that information. So, we encourage you to send in those evaluation forms or give them to us. Thank you and have a safe trip home.

DEPARTMENT OF INFORMATION SERVICES
THE COUNCIL FOR EXCEPTIONAL CHILDREN

THE ERIC CLEARINGHOUSE ON
HANDICAPPED AND GIFTED CHILDREN

AGENDA
1988 OSEP RESEARCH PROJECT DIRECTORS' CONFERENCE

JULY 10-12, 1988
DUPONT PLAZA HOTEL
WASHINGTON, D.C.

Sunday, July 10

5:00 - 7:30	REGISTRATION AND CASH BAR (Dupont Room)
5:00 - 6:30	PUBLICATION AND DISSEMINATION: CONVERSATION WITH JOURNAL EDITORS Jim Ysseldyke - EC Lynn Fuchs - JSPED Douglas Fuchs - JSPED Herb Rieth - JSPED Technology Bob Horner - JASH Sam Odom - JDEC Rebecca Fewell - TECSE Charles Greenwood - ETC, JABA Jay Gottlieb - AJMR Jim Gallagher - JEG

These journal editors will be available for informal discussions.

Monday, July 11

8:00 - 8:30 am	BREAKFAST AND REGISTRATION (Embassy Foyer)
8:30 - 8:45	INTRODUCTION AND LOGISTICS (Embassy A)
8:45 - 9:30	KEYNOTE ADDRESS - SOCIAL CONTEXT OF RESEARCH DESIGN: "From Theory to Practice in the Care and Education of Retarded Individuals." (Embassy A) SPEAKER: Edward F. Zigler Sterling Professor of Psychology, Yale University Director, Bush Center in Child Development and Social Policy
9:30 - 10:00	QUESTIONS

THE COUNCIL FOR EXCEPTIONAL CHILDREN OPERATES THE ERIC CLEARINGHOUSE ON HANDICAPPED AND GIFTED CHILDREN UNDER A CONTRACT WITH THE OFFICE OF EDUCATIONAL RESEARCH AND IMPROVEMENT, U.S. DEPARTMENT OF EDUCATION

1920 Association Drive, Reston, Virginia 22091 • (703) 620-3660

10:00 - 10:15 BREAK

10:15 - 12:00 SMALL GROUP SESSIONS

A1. Using Lap Top Computers for Data Collection
Leader: Judith Carta (Embassy B)

A2. Policy Analysis and Research (Embassy A)
Leader: Joseph Stowitschek

A3. Multivariate Analysis (Dupont 1)
Leader: Don McKinney

A4. Single-Subject Statistical Applications
Leader: Doug Marston (Dupont 2)

A5. Curriculum-Based Assessment (Dupont 3)
Leader: Stanley Deno

A6. Sequential Analysis (Board Room)
Leader: Charles Greenwood

A7. Meta-Analysis (Executive Room)
Leader: Lynn Fuchs

A8. Longitudinal Data Collection (Capital Room)
Leader: Craig Ramey

A9. Affecting Policy & Practice (Plaza Room)
Leader: Jim Gallagher

A10. Writing for Publication (Plaza Registry)
Leader: Jim Ysseldyke

12:00 - 1:30 LUNCH (Embassy A)

ADDRESS: "The Influence of Research on Policy
in Special Education."

SPEAKER: G. Thomas Bellamy
Director, Office of Special Education
Programs

1:30 - 3:00 SMALL GROUP SESSIONS

B1. Interpretive Inquiry: Naturalistic and Ethnographic Methods (Embassy B)
Leader: Julie Neururer

B2. Evaluation (Embassy A)
Leader: Ron Yoshida

B3. Meta-Analysis (Dupont 1)
Leader: Lynn Fuchs

B4. Longitudinal Data Analysis (Dupont 2)
Leader: Hill Walker

B5. Development & Validation of Instructional Packages
Leader: Scott McConnell (Dupont 3)

B6. Instrument Validation (Board Room)
Leader: Sam Meisels

B7. Strategies for Designing Interventions
Leader: Doug Carnine (Executive Room)

B8. Issues in Early Childhood Special Education
Leader: Carl Dunst (Capital Room)

B9. Affecting Policy and Practice (Plaza Room)
Leader: Jim Gallagher

B10. Issues in Secondary Education and Transition
Leader: Gene Edgar (Plaza Registry)

3:00 - 3:15 BREAK

3:15 - 4:00 GENERAL SESSION: NATURALISTIC INQUIRY (Embassy A)

SPEAKER: Yvonna Lincoln
Peabody College for Teachers
Vanderbilt University

4:00 - 4:30 QUESTIONS

4:30 - 5:30 DISCUSSION BY COMPETITION/INTEREST AREA

Researchers working on the same or related competitions or with similar interests have this opportunity to meet informally. No group leaders have been assigned. Attendees may meet in the following groups or break into smaller groups:

LD/Mildly Handicapped	(Embassy A)
Transition to Work	(Dupont 1)
Early Childhood	(Dupont 2)
Policy	(Dupont 3)
Regular Education Classrooms	(Board Room)
Technology	(Executive Room)
Social Skills	(Capital Room)
Speech/Language/Hearing Impaired	(Plaza Room)

5:30 - 7:30 CASH BAR (Embassy B)

7:30 - STUDENT/RESEARCHER DINNER

OTHERS - Dinner on your own.

Tuesday, July 12

8:00 - 8:30 BREAKFAST (Embassy Foyer)
MEET WITH PROJECT OFFICERS

8:30 - 9:30 PANEL DISCUSSION: WHAT IS EFFECTIVENESS?
(Embassy A)

MODERATOR: Naomi Zigmond

PANELISTS: (20 minutes each)
Phil Strain
Gene Edgar
Jim Gallagher

9:30 - 10:30 QUESTIONS/DISCUSSION

10:30 - 10:45 BREAK

10:45 - 12:00 SMALL GROUP SESSIONS

C1. Curriculum-Based Assessment (Embassy B)
Leader: Stanley Deno

C2. Development and Validation of Instructional Packages (Embassy A)
Leader: Scott McConnell

C3. Interpretive Inquiry: Naturalistic and Ethnographic Methods (Dupont 1)
Leader: Julie Neururer

C4. Issues in Early Childhood Special Education
Leader: Carl Dunst (Dupont 2)

C5. Dropout Research/Definitions (Dupont 3)
Leader: Naomi Zigmond

C6. Ethics in Research (Board Room)
Leader: Ann Kaiser

C7. Writing for Publication (Executive Room)
Leader: Jim Ysseldyke

C8. Issues in Secondary Education and Transition
Leader: Gene Edgar (Capital Room)

C9. Issues in the Assessment of Families
Leader: Mark Wolraich (Plaza Room)

C10. Working with Large Data Bases
Leader: Herb Rieth (Plaza Registry)

12:00 - 1:30 LUNCH (On your own. Groups with similar interests are encouraged to get together.)

1:30 - 2:30 GENERAL SESSION: VIEW FROM OSERS (Embassy A)

SPEAKER: Martin J. Kaufman, Director
Division of Innovation and Development
Office of Special Education Programs

2:30 - 3:00 QUESTIONS/DISCUSSION

3:00 - CLOSING/INFORMAL MEETINGS WITH PROJECT OFFICERS (Optional)